

**Towards an anatomy of protracted scientific  
controversy: perpetuated negotiation in the  
'directed mutation' debate**

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## **Abstract**

In 1988 an article from the Harvard School of Public Health sparked the ‘directed mutation’ debate. Its authors claimed that bacteria starved of their accustomed food were able to specifically control their genetic mutations and so adapt directly to use alternative food sources. Apparently, they were not at the mercy of random mutation to achieve adaptation as Neo-Darwinian theory demands. Rather, the authors claimed the bacteria chose their mutations and participated in their evolution as Lamarckian theorists had previously supposed.

The controversy that followed was comprised of two sub-debates. The first concerned negotiation of this molecular genetic anomaly in bacteria. The second concerned the broader debate between Lamarckians and Darwinians, and contributed a new episode to a considerable historical legacy of similar dissent.

The directed mutation debate has been protracted. I argue that it has been prolonged by active factors, which I refer to as ‘perpetuating forces’. These include: the historical and cultural context of the controversy, the influence of scientific dogma on the evaluation of the anomalies, the role of defamation by association in Lamarckian resurrections, the interdisciplinary contest for authority and participants’ styles of advocacy. I also analyse the role of the Internet in the protraction of this debate and provide quantitative analysis of the scale change caused by uptake of the debate in what I term ‘the Internet forum’.

To enable this analysis I apply boundary theory and the cartographic metaphor. I extend that theory in line with its’ architects recommendations; addressing the concept of ‘old maps’, and identifying boundary work at the interdisciplinary boundaries within the territory ‘science’.

**I, Louise Hannah Mary Jarvis, confirm that the work presented in this thesis is my own. Where information has been derived from other sources, I confirm that this has been indicated in the thesis.**

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Figure 1: Information panel on 'the walk of life' at Chester Zoo in 2002

**Reads:** 'Earlier, a scientist called Lamarck suggested that living things inherited features / characteristics developed during the lifetime of their parents. Lamarck believed that giraffes inherited long necks from ancestors who physically stretched them while browsing on trees.

Darwin thought that this was untrue. He argued that giraffes evolved long necks from ancestors who happened to have slightly longer necks. Those acquired more food, survived better, and passed on 'long neck' genes to their offspring.'

‘...what becomes of he who deals the king a non-lethal blow?’

Franklin Stahl, 1990

## Introduction

In this thesis I examine a range of hypotheses in relation to one primary and several ancillary case studies of scientific controversy. The primary case study is the directed mutation debate. That debate began in 1988, when a team of molecular biologists from the Harvard School of Public Health reported an anomaly in the mutational processes and evolution of *Escherichia coli* bacteria subjected to starvation conditions.<sup>1</sup> The team, led by eminent scientist John Cairns, claimed that the bacteria they had observed were able to control, or determine, the kinds of mutation they experienced in these conditions. It appeared that the bacteria were able to direct their genetic changes specifically towards the achievement of adaptation to use alternative available food sources. The team labelled this process ‘directed mutation’.

Neo-Darwinian theory demands that mutation be understood as a random process, not guided in any way by the demands of the environment; the mutations that arise within organisms do so with no regard for utility. In Darwinian theory, organisms that face an environmental challenge are at the mercy of random mutation for the achievement of an adaptation that allows them to survive. Thus, the Harvard claim was not compatible with orthodox Darwinian evolutionary theory. Furthermore, Cairns’ team framed their observations in Lamarckian language, adding still greater unorthodoxy to their claims. The advocates of directed mutation claimed that the cells they had observed participated in their evolution, even going as far as to suggest that the cells ‘chose’ their mutations. The negotiation of the validity of the teams’ observations, and of the validity of their non-Darwinian and possibly Lamarckian claims, formed a debate that has continued for almost two decades.

The project has six key objectives:

- i) To provide the first complete historical narrative describing the directed mutation debate.

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<sup>1</sup> Cairns, J., Overbaugh, J. & Miller, S. (1988) The origin of mutants. *Nature*, 335: 142-145.



- ii) To identify protracted scientific controversies as having particular characteristics, structure and dynamics which differentiate them from other kinds of conflicts.
- iii) To demonstrate that some active factors, which may be common to many controversies, can contribute to the perpetuation of the negotiation phase. And to show that it is these factors which confer a special/common quality upon protracted controversies.
- iv) To assess the usefulness of some existing methodological tools to the specific study of protracted controversies. To suggest modifications to some of those methodological tools, with the goal of enhancing their usefulness in the study of perpetuated negotiation. In particular, to explore potential modifications of Boundary Theory and cultural cartography.
- v) To recommend a methodology suited to the specific study of protracted scientific controversies and perpetuated negotiation.
- vi) To examine the role of the Internet in contemporary scientific controversies, particularly regarding its role as an agent of controversy perpetuation.

## **I.1 Project theses**

In this project I make four central claims, that are discussed throughout and unite the chapters:

1. I argue that protracted scientific controversies have a particular character that distinguishes them from other kinds of conflict. I suggest that certain features of their structure, dynamics and participation contribute to and determine their protraction, and that as a consequence a wide range of protracted controversies might share certain common qualities. I suggest it might be possible to identify a 'general anatomy' of protracted controversy, but argue that existing methodologies are often poorly suited to describe that anatomy since they prioritise resolution events over negotiation processes.

2. I argue that the protraction of the directed mutation debate, and possibly also that of other scientific debates, is determined by a number of

*active* factors. In this project I identify a range of these and refer to them as ‘perpetuating forces’. The perpetuating forces named and discussed here are those that appear to have had the most significant role in the protraction of the directed mutation debate. However, I have aimed to classify these forces in groups broad enough that they might be transferable as categories for identifying perpetuating factors in other case study conflicts. Therefore, the perpetuating forces I consider are:

- a) Historical legacies (Chapter 3.1)
- b) The use of ‘old maps’ of cultural authority (Chapter 3.2)
- c) Scientific dogma (Chapter 3.3)
- d) The clash of sub-disciplines (Chapter 4.1)
- e) Advocacy of one or more key individuals (Chapter 4.2)
- f) The uptake of debate in the Internet forum (Chapter 5)

I describe how each of these factors has determined the protraction of the directed mutation debate and, using supporting case studies, I indicate how each might act more broadly in protracted controversies. I suggest that a useful methodology for analysing controversy perpetuation would necessarily focus on these factors.

3. I argue that the directed mutation debate cannot be successfully analysed if viewed as either local or episodic. Rather than being a defined incidence of conflict in science, it is instead one conflict event in a series of similar and/or related contests. I argue that extrinsic factors of cultural and social context have been at least as important in this debate as intrinsic factors. I demonstrate links between this contest and those that provide its historical legacy, showing that they are bound together. I argue that existing methodology is often unsuited to explore this complexity, and that as a result the symmetry of controversy accounts suffers. I suggest that building a study of every aspect of a controversy, including its history and broader context is essential to reaching an understanding of its structure and dynamics.

4. I argue that scientific conflicts that develop a strong Internet presence take on a character that distinguishes them from debates that are conducted only in the already clearly defined ‘constitutive’ and ‘contingent’ forums of scientific debate. I suggest that the Internet represents a new forum for debate; that has its own qualities and must be recognised as an additional context for negotiation if we are to understand the structure and dynamics of contemporary scientific conflicts. I suggest that, in order to analyse the activity in this forum we require a new methodology. I demonstrate that many of the structures and visual/language tools in the existing methodology fail to translate to the conditions in that forum. I suggest that a new methodology would need to account for the large community involved in Internet-based debate, the complex groupings within that larger community, the interaction between specialist and non-specialist groups, and the classification of the various ‘qualities’ of contribution that those groups make.

## **I.2 Introducing terminology**

To enable this discussion I introduce the following terms:

- a) Perpetuating force – to describe any of the active factors that I identify as the agents of protraction in this controversy.
- b) Loud vs. Quiet advocacy – to describe individuals’ different styles of advocacy during controversies.
- c) The Internet Forum – to describe the space for debate that is represented by Internet publication. I add this to the categories ‘contingent forum’ and ‘constitutive forum’ that have already been identified as spaces for debate by Harry Collins and Trevor Pinch. I suggest that it combines several of their features, along with some that are particular to it, in a novel way that makes it a new forum in its own right.

## **I.3 Project methodology**

Chapter 1 identifies the key methodological tools available for the study of controversy, and describes the combined methodology that I have used in this

project. In this chapter I describe the transition between essentialist and constructivist accounts of science that has taken place in science studies and suggest that a methodology created from a combination of the existing approaches is best suited to enable the analysis undertaken here. I argue that even the essentialist accounts, which have been criticised regarding their demarcation criteria, can remain useful to the study of controversy if their visual or language tools are put to work in abstraction from their essentialist premise. I identify boundary work as the most broadly useful methodology for this project. In particular, I suggest that it is ideal for the analysis of the legacy of conflict between Lamarckians and Darwinians that underlies the directed mutation debate. Thomas Gieryn has suggested that boundary theory might be developed through attention to the concepts of ‘old maps’ and ‘interdisciplinary boundary work’. In this thesis I explore and develop those two extensions to boundary theory.

➤ ‘Old maps’: In Chapter 1, I describe how boundary theory assumes that the delineation and associated authority of any cultural space is constantly being drawn and redrawn during every episode of conflict. The local and episodic identity of cultural spaces and authority relations is fundamental to the theory. However, in the case of the history of Darwinian theory we see that its cultural authority has experienced a degree of persistence between conflict episodes. Gieryn acknowledges that certain cultural spaces can become ‘stabilized’, as unquestioned tacit assumptions are replicated between authority contests. Both Darwinism and Lamarckism seem to be ideal examples of the kind of ‘uncontested old maps’ that Gieryn considers this process might produce. In Chapter 3 I explore the idea of ‘old maps’ as a way of penetrating the complex status of Darwinism and Lamarckism throughout the 20<sup>th</sup> century and into the 21<sup>st</sup>.

➤ ‘Interdisciplinary boundaries’ In Chapter 1, I acknowledge Gieryn’s statement that boundary theory has chiefly addressed the delineation of science ‘from something else altogether’, rather than examining the delineations that determine the boundaries between the various sub-disciplines within the cultural territory ‘science’. In Chapter 4, I argue that the concepts and categories of

boundary work that have already been developed in the study of the delineation of science from other cultural territories can equally be transferred to describe the conflicts that occur within science and determine the delineations of the sub-disciplines. I describe the conflict between evolutionary biologists and molecular biologists, which is at the heart of the directed mutation debate, using the terms that have already been developed and put to work in boundary studies of the delineation of the territory 'science'. I suggest that the same activities are apparent at the interdisciplinary boundary as at the disciplinary boundary. I argue that the same tools of cartography are useful to understanding these delineations, but that a larger scale map is needed to depict the internal world of the cultural territories in more detail.

Various other methodological tools are applied in specific areas of the project. Of particular importance are:

- Collins' core-set terminology and Empirical Programme of Relativism (EPOR). In Chapter 2 these are used together to describe the structure of the early part of the directed mutation debate (1980-1990) and to order the participants in the journal-based phase of the negotiation.
- Patricia Fara's classification of the qualities of 'genius'. This is employed in Chapter 3 as a tool to identify the tactics used to raise Charles Darwin as an icon during the 1959 centennial celebrations.
- Betty Smocovitis' analysis of the functions of celebration. This is discussed in Chapter 3 in relation to boundary work function of the centennial celebrations.
- Kuhn's theory of scientific revolutions and Lakatos' theory of research schools. These are discussed to reveal the different activity and approaches in molecular biology and evolutionary biology in Chapter 4.1.

- Irving Langmuir's theory of pathological science. This is employed in Chapter 4.2 to evaluate whether Cairns' style of advocacy qualifies as 'pathological'.
- The various cartographies of cyberspace that visualize the Internet. In Chapter 5, I compare the depiction of adjacency in those cartographies with the representations of adjacency, and thus authority, that are recorded in Gieryn's cartography.

#### **1.4 Project structure**

Chapter 1 has two principal aims; first, to provide an overview of the many existing approaches to scientific controversy and the methodologies that they rely upon, and second, to outline this project's methodology, which I have synthesised from elements of several of those existing approaches. The chapter describes the trend away from essentialist and philosophical explanations of science and scientific activity towards more constructivist and sociological accounts. It describes how, for some authors, questions about the nature of science have shifted their focus from the 'demarcation' problem to the 'boundary' problem. In this chapter I identify the 'end-directedness' common to many accounts of scientific controversy, and highlight this as an impediment to the study of protracted controversy.

Chapter 2 is a detailed case study of the structure and dynamics of the directed mutation debate, from its inception in the 1980s, through to the present day. The historical narrative is constructed from the primary materials of the journal-based debate, the Internet-based debate, and oral history accounts. This case study has two functions in relation to my analysis: First, to provide the basis for a discussion of the structure and dynamics of the debate. Second, in association with other supporting case studies, to launch a discussion of some more general issues relating to the nature of scientific controversy. The use of a specific case study as the basis for a more general analysis of scientific controversy follows a good deal of precedent.

Chapter 3 addresses three of the six perpetuating forces that I describe in this project: historical legacies, old maps and scientific dogma. I argue that these aspects of the broader context of the debate influenced the participants, and affected their interactions and response to the challenge that directed mutation presented. In this chapter, I describe how the directed mutation debate arose within the context of significant established attachment to Darwinism coupled with significant established antagonism to Lamarckism. I describe how that broad context for the debate shaped its character, influenced its participants, and ultimately protracted the negotiation phase and impeded closure.

Chapter 4 deals with a further two of the six perpetuating forces: the clash of sub-disciplines and the role of advocacy. I argue that one of the principal forces of protraction of the directed mutation debate has been that it engaged two disciplinary groups: molecular biologists and evolutionary biologists. (4.1) I also argue, that the debate has been actively protracted by individuals, on account of their chosen styles of advocacy. (4.2)

In Chapter 5 I argue that the Internet represents a new, important and poorly understood forum for scientific debate, and that understanding the conditions of that forum will be essential to understanding the quality of contemporary scientific debates. This chapter describes the many contributions that comprise the directed mutation debate on line. I argue that at the point of uptake the scale of the debate increased dramatically, and the meanings of the debate diversified. The new audiences and participants changed the terms and meanings of the debate, so that many closures were needed rather than just one. In this chapter I ask whether closure is even possible in the kinds of diversified debates that the Internet encourages.

## **Chapter 1: Methodology and Literature Review**

This chapter seeks to locate this project's content and methodological approach as a contribution to the ongoing endeavour to understand scientific controversy. A wealth of literature describes the structure and dynamics of that aspect of science. This chapter categorizes and evaluates that literature, focussing chiefly on the technical resources that I have selected and combined to create the methodology of this project. I suggest that a methodology synthesised from aspects of a wide variety of the existing approaches is the most appropriate to this study of protracted controversy.<sup>2</sup>

Many analyses of scientific controversy have emerged from attention to the 'demarcation' problem.<sup>3</sup> That is to say, they have been generated as part of investigations of the means by which: i) science has achieved and maintained its special status in relation to society throughout history, ii) science has been perceived as a more likely and/or abundant source of 'truth' and knowledge than other forms of intellectual or creative work, and iii) scientists and the public determine the distinction between science and non-science, and between scientific orthodoxy and unorthodoxy. There is a long history of attention to these issues from philosophers, sociologists and historians, in which the changing perceptions of and approaches to scientific controversy are embedded.

Section 1.1 provides an overview of the various approaches that have constituted that history, focussing on how the 'demarcation problem' came to be reinterpreted as the 'boundary problem' in more recent sociological treatments.

Section 1.2 describes a selection of models and analyses that are usually classified as philosophical, or perhaps 'essentialist'. I argue that despite the essentialist critique, which has rendered such theories less popular, elements of their language and visual tools remain useful in the context of a synthetic methodology

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<sup>2</sup> An account of the research method used in this project appears in Appendix 1, alongside a description of the conception and design of this thesis.

<sup>3</sup> See Gieryn, T. (1983) Boundary work and the demarcation of science from non-science: strains and interests in professional ideologies of scientists. *American Sociological Review*, 48, 781-795 and Gieryn, T. (1995) Boundaries of science. In: S. Jasanoff, G. E. Markle, J. C. Petersen and T. Pinch (Eds) *Handbook of Science and Technology Studies*. Sage Publications, Thousand Oaks, CA.



that employs them in abstraction from their essentialist premise. I describe those elements that I have selected to contribute to the combined methodology of this project.

Section 1.3 explores a selection of approaches that are sociological, or perhaps 'constructivist'. I focus particularly on the concepts of 'boundary work' and the 'cartographic metaphor' as outlined and explored by sociologists Thomas Gieryn<sup>4</sup>, Harry Collins and Trevor Pinch<sup>5</sup>, and historians Steven Shapin and Simon Schaffer<sup>6</sup>. In this thesis I argue that boundary work methodology provides an excellent tool for the study of the historical conflict between Lamarckism and Darwinism, but that it requires modification to be equally useful in the more specific study of the directed mutation debate. Thus, in section 1.3 I outline some modifications that might extend the methodology to describe contests *within* science as well as contests between science and other domains. I also discuss the notion of 'old maps' (which Gieryn has identified as a topic for further research<sup>7</sup>), and their relevance to the history of Darwinism and Lamarckism.

In section 1.4 I describe some treatments of scientific controversy that have emerged from contexts other than those associated with explicit attention to the demarcation and boundary problems. These include controversy analyses generated from policy, politics and economics studies, as well as those emerging from popular accounts of scientific controversy. I identify some methodological contributions that these treatments might make to the analysis I undertake in this project.

Section 1.5 explores the use of history and the role of narrative in controversy analyses. This section highlights precedent for, and merits of, the 'case study-based' approach that this project adopts.

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<sup>4</sup> Gieryn, T. (1995)

<sup>5</sup> See for examples of Collins' and Pinch's treatment of the 'boundary problem': Collins, H. & Pinch, T. (1979) *The construction of the paranormal: nothing unscientific is happening*. In: R. Wallis (Ed.) *On the margins of science*. Sociological Review Monographs 27, University of Keele Press, Keele; Collins, H. & Pinch, T. (1993) *The Golem: what you should know about science*. Cambridge University Press, Cambridge.

<sup>6</sup> Shapin, S. & Schaffer, S. (1985) *Leviathan and the air pump: Hobbes, Boyle, and the experimental life*. Princeton University Press, Princeton, NJ.

<sup>7</sup> Gieryn, T. (1999a) *Cultural boundaries of science: credibility on the line*. University of Chicago Press, Chicago.

In section 1.6 I identify the ‘end directed’ trend manifest in many of the controversy treatments described in this chapter. I describe the context for the emergence of that trend and highlight the disadvantages of its pervasive influence. The problems of end-directed identities of scientific controversy are discussed, in particular the problem of ‘asymmetry’. I identify ‘boundary work’ methodology as one approach that might potentially avoid these problems and thus enable a more revealing and symmetrical discussion of protracted or perpetuated controversies.

Finally, in section 1.7 I show that adopting the notion or language of ‘perpetuating forces’ relieves the problem of end-direction. Perpetuating forces allow controversy to be reframed with ‘activity’ emphasised over ‘outcome’. This section describes how my project analyses the directed mutation debate in relation to possible examples of perpetuating forces *in action*. I argue that the concept of perpetuating forces enables a particularly rich and clear interpretation of the circumstances of that debate.

Throughout this chapter I explore the argument that reliance on the newer sociological tools, in the absence of comparison and contrast with the older philosophical tools and the less explicit tools of historians, results in analyses of controversy that lack some of the clarity and richness that might be achieved through a more integrated methodological approach.

### **1.1 Modelling conflict: from ‘demarcation’ to the ‘boundary problem’.**

The ‘demarcation problem’, or the ‘boundary problem’ as we might term it in line with more recent treatments<sup>8</sup>, asks: ‘Where does science leave off, and society – or technology – begin? Where is the border between science and non-science?’<sup>9</sup> There is a long tradition of attention to these questions. In asking how science has come to achieve and retain its special status in relation to other

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<sup>8</sup> See Barnes, B. (1974) *Scientific knowledge and sociological theory*. Routledge and Kegan Paul, London for the ‘setting up’ of the ‘boundary problem’ and Gieryn, T. (1995) for discussion of the boundary problem and various approaches to it.

<sup>9</sup> Gieryn, T. (1995) p.393

knowledge-producing or creative activities, theorists have inevitably faced the problem of trying to define science and its demarcation from other kinds of activity. The search for that definition has driven theorists to identify the 'boundaries of science', and to consider how those boundaries are policed and redrawn, and therefore, how the scientific community demarcates orthodoxy from unorthodoxy.

The models and analytical materials produced often address conflict in science, since episodes of controversy seem to provide clear and abundant examples of demarcation or boundary work activities. As a result, the literature is dominated by descriptions of how the 'mainstream' or 'establishment' responds to unorthodox researchers or anomalous findings as it seeks to protect its authority. The processes of marginalization and discredit are common themes. Some analyses focus on the ways in which knowledge is affected when orthodoxy and unorthodoxy or even science and non-science clash. Others focus on the behaviour and motivation of the participants in these conflicts.

Many of these analyses are fraught with problems of asymmetry. They assume a predetermined or predefined identity of orthodoxy and unorthodoxy, and assume a dynamic between the two that privileges orthodoxy over the unorthodox. To promote symmetry, analyses of conflict must abandon the *a priori* assumptions embodied in those categories. There is a tendency or temptation to look at controversies teleologically, and make analyses of them based upon which parties it seems turned out to be 'right' at the end of the conflict (see section 1.6). A symmetrical approach instead focuses attention on the reasons that certain parties had for assuming their position, and attempts to show the force of those reasons other than in relation to the outcome of the conflict. As philosopher and sociologist Sergio Sismondo explains, the symmetrical approach 'attempts to recover rationality in controversies, where the rationality of one or another side is apt to be dismissed or forgotten.' A symmetrical approach would view each episode of demarcation or boundary work as part of an ongoing negotiation of very moveable boundaries. In symmetrical terms the categories 'orthodox' and 'unorthodox' are re-determined during each episodic or local event that comprises that ongoing negotiation. Theories and analyses borne out of constructivist attention to the boundary problem promote a

stronger degree of symmetry than the studies of demarcation based on an essentialist perspective. The newer sociological approaches abandon the *a priori* assumptions of the essentialist view of science, and therefore are better positioned to achieve a symmetrical approach to controversy studies. Of the approaches to controversy discussed here, Thomas Gieryn's boundary work methodology provides perhaps the best opportunity for symmetrical analysis (as described in section 1.3). The symmetry principle is discussed further below.

Before discussing and categorising various treatments of controversy, it is useful to appreciate the context of their generation. In particular, it is important in terms of this thesis to recognise the degree to which constructivist sociological models of science and scientific controversy have challenged the older philosophical approaches to the demarcation of science.<sup>10</sup> This project advocates the use of a methodology synthesised from elements of both the essentialist and constructivist accounts, and so it is useful to understand their relationship to each other.

A significant attempt to resolve the demarcation problem was mounted during the 1930s by the group of philosophers that constituted the Vienna Circle. Those philosophers developed the 'logical positivist' approach to define the character of science in relation to other activities.<sup>11</sup> Logical positivism maintains that scientific theories are established through an inductive process that transforms accumulated data points into general statements. A scientific theory therefore is 'a mere summary of possible observations, in a logically structured language'.<sup>12</sup> The methodology of science appears to ensure the success of this inductive process since it provides frameworks 'in which it is possible to unequivocally generalize from data'.<sup>13</sup> Logical positivism, like the philosophical theories that were to follow, was essentialist. It identified the special character of science as an intrinsic quality, in this case conferred by the methodology of science itself.

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<sup>10</sup> Gieryn, T. (1995)

<sup>11</sup> Sismondo, S. (2004) *An introduction to science and technology studies*. Blackwell Publishing, Malden, MA.

<sup>12</sup> Sismondo, S. (2004) p.2

<sup>13</sup> Sismondo, S. (2004) p.2

During the 1960s the philosopher Karl Popper proposed alternative demarcation criteria.<sup>14</sup> Popper had been on the margins of the Vienna Circle and retained their essentialist perspective. However, he criticised their inductivist approach, stating that no amount of observations could ever allow extension from a finite number of cases to a generalised rule 'true' of all cases. Instead of identifying the special nature of science as based upon the accumulation of observations, Popper suggested that a true scientific theory was 'falsifiable'. A falsifiable theory only makes predictions that are open to question, or for which a potential disproof can be imagined. Science is, in that view, not an inductive process, but rather a creative process of theoretical conjecture combined with scepticism.

Other theorists attempted to move beyond the formulation of demarcation criteria focused on the relations of data, observation, evidence and truth and the intrinsic qualities of science manifest in its methodology. Some developed approaches to account for the social structure and social function of science, while retaining the essentialist view that ultimately an intrinsic quality demarcates science. For example, during the 1940s, Robert Merton began to offer contributions to the demarcation problem from the perspective of sociology.<sup>15</sup> For Merton, an institutionalized ethos replaced falsifiability as the keystone of scientific work.<sup>16</sup> In his view, society is composed of a series of interacting institutions, each of which fulfils a particular function and contributes to 'the stability and flourishing of the society'.<sup>17</sup> Merton perceived science as having the institutional goal of extending certified knowledge.<sup>18</sup> His was a functionalist view that saw science as a social and cultural structure that relied upon certain behavioural norms to make the fulfilment of its goal a more likely outcome. Merton identifies four norms: Communism, Universalism, Disinterestedness and Organised scepticism.<sup>19</sup> Sociologist Thomas Gieryn has said that if Merton's norms are read as demarcation criteria 'then knowledge-producing activities not ensconced in that institutionalized moral frame

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<sup>14</sup> See Popper, K. (1963) *Conjectures and refutations*. Routledge & Kegan Paul, London.

<sup>15</sup> Merton, R. (1942) The normative structure of science. In: N. Storer (Ed.) (1973) *The sociology of science: theoretical and empirical investigations*. University of Chicago Press, Chicago.

<sup>16</sup> Gieryn, T. (1995)

<sup>17</sup> Sismondo, S. (2004) p.20

<sup>18</sup> Merton, R. (1942) p.270

<sup>19</sup> Merton, R. (1942); and for discussion of the four 'norms' see Gieryn, T. (1995)

must be non-scientific'.<sup>20</sup> So, Merton's science is still essentialist in that an unalienable character defines it, although, the criteria for distinction are social rather than epistemic.

In 1962, historian Thomas Kuhn published his much celebrated work, *The Structure of Scientific Revolutions*<sup>21</sup>, in which he described his impression of the process of scientific change and 'progress'. The impact of his contribution has in turn been referred to as 'the Kuhnian revolution'.<sup>22</sup> Kuhn's analysis, while following Merton in taking account of social factors, was still roughly essentialist. Like his predecessors, Kuhn still sought the intrinsic features of science that defined its nature. However, as Gieryn points out, Kuhn did this with a degree of self-awareness, stating: 'we must not, I think, seek a sharp or decisive 'demarcation criterion''.<sup>23</sup> Gieryn qualifies Kuhn's attention to the value of demarcation stating that:

'Still, looking back on Kuhn from a perspective shaped by 10 years of constructivist empirical studies of boundary-work, a case can be made that he set that line of inquiry in motion more as a foil than as a pioneer'.<sup>24</sup>

Kuhn's analysis assumed that Popper's concept of a constant process of conjecture and falsification was not a true reflection of scientific activity, at least not during the phase that Kuhn termed 'normal science'.<sup>25</sup> <sup>26</sup> In place of Merton's 'social norms' Kuhn introduced 'the moral force of cognitive norms',<sup>27</sup> with science based upon consensus. He thought that consensus allowed 'paradigms' to be formed, representing the agreed theoretical framework for investigations. In his theory, when

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<sup>20</sup> Gieryn, T. (1995) p. 399

<sup>21</sup> Kuhn, T. (1962) *The structure of scientific revolutions*. Chicago University Press, Chiacgo.

<sup>22</sup> Sismondo, S. (2004) p.12

<sup>23</sup> Kuhn, T. (1970) (Rev.2<sup>nd</sup> Ed.), *The structure of scientific revolutions*. Chicago University Press, Chicago p.6, cf. in Gieryn, T. (1995)

<sup>24</sup> Gieryn, T. (1995) p.401

<sup>25</sup> Kuhn, T. (1962)

<sup>26</sup> Gieryn, T. (1995)

<sup>27</sup> Gieryn, T. (1995) p.401

facts or observations arise that conflict with the paradigm they must be examined and, if the paradigm can be protected in light of them, they must be dismissed. If an anomaly arises that cannot be managed in the context of the paradigm, then Kuhn perceived that a 'scientific revolution' would result, as the old paradigm was abandoned and replaced with a new framework of consensus.<sup>28</sup>

Popper, Merton and Kuhn provide the key essentialist approaches relevant to this project. Numerous criticisms have been levelled at each of these approaches to science and the view of scientific conflicts that they expound.

For example, Popper's conflicts (falsification attempts) are criticised for being too ubiquitous, too built-in to the scientific process and too incessant.<sup>29</sup> His process of falsification relies upon the reproducibility of falsifying empirical evidence and assumes that the moment of falsification is apparent and un-negotiable to those involved. Sociologist Harry Collins has pointed out that cultural and rhetorical factors contribute significantly to scientists' choices between theories, and that, therefore, falsification is contingent and social rather than natural or purely logical.

For Collins, Popper's scheme is too idealistic, and does not allow for the social elements of science. In particular, Collins argues that there is no unambiguous way of knowing when an experiment has been reproduced effectively; the decision concerning the authenticity of a replication being open for scientists to decide. This leads to what Collins calls 'experimenter's regress', in which decisions about the success of an experimental replication are inextricably tied to negotiation of the reality of the phenomenon being tested.<sup>30</sup> As a case study illustration of this point, Collins describes the 1970s controversy surrounding physicists' attempts to measure gravitational waves. In that case, one researcher's initial claim to have detected such waves was followed by a rush of attempts at replication by other scientists at other locations. Some replications produced positive results and supported the initial

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<sup>28</sup> Kuhn, T. (1962)

<sup>29</sup> See for example: Kuhn, T. (1977) *The essentialist tension: selected studies in scientific tradition and change*. University of Chicago Press, Chicago.

<sup>30</sup> Collins, H. (1985) *Changing Order: replication and induction in scientific practice*. Sage Publications, London.

claim, while others detected no gravitational waves and so appeared to falsify the original reports. However, the negative results could be designated by the supporters of gravitational wave theory as the result of the use of non-functional equipment. The central problem is that it is only clear that an experimental system is working when it generates the desired results, and so cannot be used to obtain a negative result. This issue presents a serious problem for Popper, whose theory relies upon replication to test falsification.

Critics have argued that Merton's norms are also too idealistic and thus ill-suited to describe conflicts undertaken in the inevitable context of widespread 'interests'. Harriet Zuckerman has argued that, in all likelihood, the norms are not constant, but rather are interpreted differently by different individuals at different times and in different places; that is norms of behaviour in science are contingent rather than ideal. Zuckerman argues that in reality Merton's four norms are possibly not even common.<sup>31</sup> Similarly, sociologist and cognitive scientist Aaron Cicourel has argued that behavioural norms in science are interpretive and are negotiated collectively by actors as they approach science as part of their everyday practice. Cicourel argues against essentialism and for a more social scheme involving the negotiation and deployment of norms that have been constructed by the actors involved.<sup>32</sup>

Sociologist Michael Mulkay suggests that Mertonian type norms might exist as a kind of point of reference for the scientific community, but that the way the community interacts with and relates to the norms at any given time is socially determined and contingent on a combination of factors. Mulkay considers that, at any time, interests will have an impact upon the way the norms are interpreted and acted upon. In Mulkay's view, the norms are idealistic and can only provide a general point of reference when in the context of the social situations that constitute science as work.<sup>33</sup> Furthermore, Mulkay has argued that there is little evidence of

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<sup>31</sup> Zuckerman, H. (1977) Deviant behaviour and social control in science. In: E. Sagarin (Ed.) *Deviance and social change*. Sage Publications, London; Zuckerman, H. (1984) Norms and deviant behaviour in science. *Science, technology and human values*, 9: 7-13.

<sup>32</sup> Cicourel, A. (1974) *Cognitive Sociology*. Free Press, New York.

<sup>33</sup> Mulkay, M. (1969) Some aspects of cultural growth in the sciences. *Social Research*, 36:22-52; Mulkay, M. (1980) Interpretation and the use of rules: the case of norms in science. In: T. Gieryn



any rewards imperative of the Mertonian kind in science, or that conforming to the norms is linked, or perceived to be linked, to any system of rewards.<sup>34</sup>

Ian Mitroff has gone further, and identified a series of counter-norms that he claims exist in science in tension with the Mertonian norms. He argues that, the flexible combination and interpretation of these norms and counter-norms determines social behaviour in science. Mitroff based his counter-norms theory on interviews with 42 scientists involved in the Apollo moon project. He discovered that as well as valuing the four Mertonian norms, the scientists also thought that behaviours such as secrecy and emotional commitment were important in scientific activity. Mitroff lists six anti-norms: non-rationality, emotional commitment, particularism, solitariness, interestedness and organized dogmatism.<sup>35</sup>

Finally, Kuhn's conflict episodes have been criticised as too uniform and violent, and as assuming that too high a level of unpartisan objectivity determines the scientific community's approach to unorthodox assaults on paradigmatic theory. Nigel Gilbert and Michael Mulkay have highlighted a fundamental problem with Kuhn's paradigm and revolution scheme. They argue that the kind of widespread consensus that is vital to the operation of a paradigm is not apparent in the scientific community, suggesting that the reality of widespread discrepancies in the beliefs of individuals and sub-communities renders Kuhn's 'normal' science untenable and unrealistic. Rarely is the scientific community so agreed on any issue as to be in a state of absolute consensus.<sup>36</sup> Also, Kuhn's notion of incommensurability between different paradigms seems to be similarly idealistic. Kuhn suggested that scientists working with different paradigms effectively inhabit 'different worlds'.<sup>37</sup> Several critics have pointed out that the history of science betrays a much greater continuity

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(Ed.) *Science and social structure: a festschrift for Robert Merton*. Transactions of the New York Academy of Sciences, Series II, Vol.39. New York Academy of Sciences, New York.

<sup>34</sup> Mulkay, M. (1975) Norms and ideology in science. *Social Science Information*, 15(4/5): 637-656.

<sup>35</sup> Mitroff, I. (1974) Norms and counter-norms in a select group of Apollo moon scientists: a case study of the ambivalence of scientists. *American Sociological Review*, 39: 579-595.

<sup>36</sup> Gilbert, N. & Mulkay, M. (1984) *Opening Pandora's box: a sociological analysis of scientists' discourse*. Cambridge University Press, Cambridge.

<sup>37</sup> Kuhn, T. (1962)

between periods of science history, and that the extent of change rarely seems sufficient to satisfy the criteria for Kuhn's revolutions.<sup>38</sup>

These and other criticisms contributed to the essentialist critique that arose in the late twentieth century. Various authors encouraged an alternative view of science that took better account of the behaviours of its participants and the social settings in which it has been conducted. Rather than seeking the intrinsic quality that determined *why* science was attributed such status, theorists began to encourage us to consider *how* science had achieved that status; that is, what activities and processes of articulation have enabled science to achieve authority: they encouraged a constructivist approach. For example, in Gieryn's view, the problem with the essentialist approach is that it seeks explanations for the superiority of science, without being reflexive about the fact that science has attained such status.<sup>39</sup> Gieryn explains that the constructivist approach relieves this problem. He states:

'Constructivists argue that no demarcation principles work universally and that the separation of science from other knowledge-producing activities is instead a contextually contingent and interests-driven pragmatic accomplishment drawing selectively on inconsistent and ambiguous attributes.'<sup>40</sup>

Gieryn adds that the constructivist approach, especially when drawn away from the quest for 'demarcation criteria' to the more open analysis of the 'boundary problem', asks more about the achievement of cultural authority, than about any intrinsic quality of science. He explains that:

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<sup>38</sup> See for examples: Bird, A. (2000) *Thomas Kuhn*. Princeton University Press, Princeton, NJ; Barnes, B. (1982) *T.S. Kuhn and social science*. Columbia University Press, New York; Davidson, D. (1974) On the very idea of a conceptual scheme. *Proceedings of the American Philosophical Association*, 47:5-20.

<sup>39</sup> Gieryn, T. (1995)

<sup>40</sup> Gieryn, T. (1995) p.393

‘...attention shifts to representations of scientific practice and knowledge in situations where answers to the question, ‘what is science?’ move from tacit assumption to explicit articulation. The task of demarcating science is reassigned from analysts to people in society...’.<sup>41</sup>

During the 1970s the sociology of scientific knowledge (SSK) arose in an attempt to consolidate the social approach. The advocates of SSK suggested that the very material of scientific knowledge was itself socially contingent and constitutive. Scientific knowledge was not immune to analysis by virtue of any special quality, but could instead be analysed in the same way as other knowledge products. Traditionally, it had been suggested that science produced immutable asocial truths that were not available for analysis, with scientists involved in an ongoing dialogue with Nature through which they gained access to those truths. Scientists themselves may have been subject to sociological forces, but the knowledge they produced was immune from that activity. SSK arose as a criticism of that positivist approach to demarcation. It promoted a relativist approach to science study, in which both sides in any controversy are examined symmetrically using the same tools, and in which knowledge is a social construct rather than an immutable asocial truth.

While Merton and Kuhn<sup>42</sup> had made moves towards the social, they had done so in a way that reflected the internalist/externalist debate. They saw social factors as impinging upon the scientific process - a kind of perturbation, often stemming from an external source. In particular, it seemed to them that pseudoscience or errors in science could be explained with reference to the influence of external social factors. As Steve Shapin suggests, during the 1960s heyday of Mertonian sociology the ‘social’ was a kind of contaminant of the scientific process.

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<sup>41</sup> Gieryn, T. (1995) p.405

<sup>42</sup> Merton’s relation to SSK is apparent in as much as he was a founder of the sociology of science and so merged the social and scientific in such a way that the path was laid for the social study of scientific knowledge to emerge from the social study of scientific practice. Sociologist Barry Barnes argues that Kuhn’s contribution can also be traced in later developments in SSK. He considers that Kuhn merged science, sociology and history in his paradigm theory; the same convergence of subjects as was later promoted at Edinburgh where the strong programme of SSK arose. See: Barnes, B. (1982)

The control of the 'social' was a concern, and Merton's norms, for example, represented a means that scientists had created by which to control this influence. During this period, the sociology of science was marked by considerations of the external and internal, the rational versus the social, and the dualism of social and intellectual factors. With the birth of SSK, attention shifted to analysis of the extent to which society, rather than being a pollutant, was instead a necessary condition for the making, holding, extending and changing of knowledge products themselves.<sup>43</sup>

Early SSK was largely developed in Britain.<sup>44</sup> During the 1970s a group of philosophers, sociologists and historians in Edinburgh set out to describe the content of scientific knowledge in sociological terms. They developed the 'strong programme' as a kind of mission statement for that enterprise.<sup>45</sup> In 1976 David Bloor laid out the tenets of that programme.<sup>46</sup> He suggested that SSK should:

- i) offer causal explanations for the state of knowledge and the beliefs that develop around it.
- ii) be impartial in relation to truth or falsity, success or failure; realising that both kinds of knowledge claims require explanation.
- iii) be symmetrical i.e. assert that both true and false beliefs have the same kinds of explanation.
- iv) be reflexive i.e. recognise that the patterns of explanation they apply to science should also be applicable to sociology and its knowledge products/beliefs.

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<sup>43</sup> Shapin, S. (1995b) Here and everywhere: sociology of scientific knowledge. *Annual Review of Sociology*, 21: 289-321.

<sup>44</sup> The sociology of *scientific* knowledge (SSK) was distinct from the existing sociology of knowledge that had arisen slightly earlier. The sociology of knowledge had been pioneered by Karl Mannheim who, although rendering knowledge generally socially contingent, had stated that the 'rational methodology of the natural sciences precluded them from social explanation'. The founders of SSK wanted to extend the sociology of knowledge to cover scientific knowledge also; they wanted to be 'symmetrical' in their analyses by treating science in the same ways as other activities. They called their programme 'the strong programme' to help distinguish its methodological goals from those of Mannheim. See Fuller, S. (1992) Being there with Thomas Kuhn: A parable for postmodern times. *History and Theory*, 31(3):241-275.

<sup>45</sup> Sismondo, S. (2004)

<sup>46</sup> Bloor, D. (1976) *Knowledge and social imagery*. Routledge and Kegan Paul, London.

Investigations of the nature of scientific knowledge took several forms during the 1970s and 1980s. SSK encouraged empirical studies, and a huge amount of historical work was done in line with the strong programme. Attention was turned to the role of 'interests' and objectives in the construction of scientific knowledge. The interests model showed that scientists choose which knowledge claims to support, or which knowledge to 'believe' in, in the context of various motivations that shape their choices, the representations that they offer and eventually the knowledge constructions that they produce.

For example, historian John Dean has shown that the morphological and genetic theories of plant species distinction were accepted simultaneously in the 20<sup>th</sup> century on account of the differing interests of the scientists in herbaria and museums versus scientists in laboratories and experimental settings.<sup>47</sup> Steven Shapin has shown that the nineteenth century Edinburgh anatomy debate over the correct depiction of the human brain was fuelled by the social and political uses that were planned for those representations. Shapin links the growth of phrenology in the 1820s to contemporary heightened class struggles and the reform movement.<sup>48</sup> Also Shapin, along with Simon Schaffer, has shown that the political and cultural causes underlay the choices between knowledge claims in the debate between Robert Boyle and Hobbes concerning Boyle's air pump experiments.<sup>49</sup>

In line with the strong programme, interest analyses of this kind prioritise symmetry. Mertonian sociology was interested in social forces as external phenomena, rather than as an intrinsic part of knowledge production. Barry Barnes has pointed out that the newer interest analyses are not explicitly external or internal. For example, Dean's analysis considers the 'narrow esoteric' objectives of scientists associated with their professional objectives and so is more like an internal history of science. Shapin and Schaffer on the other hand are concerned

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<sup>47</sup> Dean, J. (1979) Controversy over classification: a case study from the history of botany. In: B. Barnes & S. Shapin (Eds.) *Natural Order*. Sage Publications, London.

<sup>48</sup> Shapin, S. (1979) The politics of observation. In: R. Wallis (Ed.) *On the margins of science: the social construction of rejected knowledge*. *Sociological Review Monograph*, 27: 139-178.

<sup>49</sup> Shapin, S. & Schaffer, S. (1985)

with the interests and objectives arising from the broader social and cultural context and so provide a more external history.<sup>50</sup>

The interest models pursued as part of SSK have also been criticised. For example, Cantor has criticised Shapin's analysis of phrenology, saying that class membership is not as clear as Shapin suggests and so cannot be linked with such clarity as a cause of knowledge choices.<sup>51</sup> Steve Woolgar argues that SSK advocates invoke interests as causes without being able to draw a clear casual relationship between the interest they identify and the knowledge outcome they link it with.<sup>52</sup> Yearley has added that this situation is exacerbated by the fact that interests are rarely straightforward or easy to identify.<sup>53</sup> In spite of this, interests remain important to the sociological analysis of science, and the interest model is particularly useful for identifying motivations during periods of controversy.

Ethnomethodology and discourse analysis have also been popular in SSK. In these analyses authors focus on specific accounts of knowledge that scientists give, for example in journal articles or in interviews. Materials are studied to show not the actions of scientists, but rather the accounts of action that scientists construct to achieve certain interpretations. In these studies, rhetoric and argument are identified as influencing decisions concerning knowledge and belief. Symmetrical analyses of discovery, that do not take account of the truth or falsity of a discovery, have been a common goal of ethnomethodological study and discourse analysis. Garfinkel, Lynch and Livingston have used tape recordings from the laboratory to analyse the discovery of the optical pulsar by Cocke and Disney.<sup>54</sup> Collins has used interviews to study scientists' attempts to build TEA lasers during the 1970s.<sup>55</sup> Through these

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<sup>50</sup> Barnes, B. (1990) Sociological theories of scientific knowledge. In: R. Olby, G. Cantor, J. Christie & M. Hodge (Eds.) *Companion to the history of modern science*. Routledge, London.

<sup>51</sup> Cantor, G. (1975) A critique of Shapin's social interpretation of the Edinburgh phrenology debate. *Annals of Science*, 32: 196-219.

<sup>52</sup> Woolgar, S. (1981) Interests and explanation in the social study of science. *Social Studies of Science*, 11: 365-394.

<sup>53</sup> Yearley, S. (1982) The relationship between epistemological and sociological cognitive interests: some ambiguities underlying the use of the interest theory in the study of scientific knowledge. *Studies in the History and Philosophy of Science*, 13: 353-388.

<sup>54</sup> Garfinkel, H., Lynch, M., Livingston, E. (1981) The work of discovering science construed with materials from the optically discovered pulsar. *Philosophy of the social sciences*, 11: 131-158.

<sup>55</sup> Collins, H. (1974) The TEA set: tacit knowledge and scientific networks. *Science Studies*, 4: 165-186.

kinds of studies the processes of attribution and credit in science have come to be better understood as a social process. Also, Collins' study enabled him to identify the craftwork elements of knowledge sharing and acquisition, and the role of tacit knowledge in science practice. However, these studies have also received criticism. Gilbert and Mulkay have criticised the interview technique in general, suggesting that the statements scientists make cannot be viewed as a source of valid information, other than that concerning scientists' patterns of discourse.<sup>56</sup> Mulkay has gone as far as to say that sociologists cannot use discourse to define or explain science because it is dependent on the very nature of scientists discourse.<sup>57</sup>

Trevor Pinch has identified the distinction between the various approaches to SSK as a difference in where they 'locate the constraining features of the social world'. For interest theorists, the social content is in communities or in the wider cultural context. For ethnomethodologists, the constructed accounts of science themselves manifest the social legacy.<sup>58</sup> Pinch points out, however, that the various approaches in SSK are not mutually exclusive, but rather can be used in combination to illuminate a particular situation from a variety of perspectives.

A key outcome of the constructivist, anti-essentialist and SSK approaches has been the redefinition of the 'demarcation' problem as the 'boundary' problem. Sociologist Thomas Gieryn has been a key force in that redefinition, and has been developing his 'boundary theory' since the early 1980s.<sup>59</sup> For Gieryn, the redefinition allows a different interpretation of science. Rather than being an enterprise with 'essential' character, science can be thought of as a cultural territory, the perimeters of which are contingent and open to review. The territory 'science' confers cultural authority, and exists adjacent to other kinds of cultural domain. Scientists negotiate and assert the location of the territorial boundaries, but they do so in unison with many other individuals, from other cultural territories, who are

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<sup>56</sup> Gilbert, G & Mulkay, M. (1984)

<sup>57</sup> Mulkay, M., Potter, J. & Yearley, S. (1983) Why an analysis of scientific discourse is needed. In: Knorr-Cetina & M. Mulkay (Eds.) *Science Observed: Perspectives on the social study of science*. Sage Publications, London.

<sup>58</sup> Pinch, T. (1990) The sociology of the scientific community. In: R. Olby, G. Cantor, J. Christie & M. Hodge (Eds.) *Companion to the history of modern science*. Routledge, London. p.95-96.

<sup>59</sup> Gieryn, T. (1983)

also interested in establishing the boundaries of science. The boundaries are located and maintained as various individuals carry out the active task of 'boundary work'. Boundaries are in general drawn and redrawn continuously, and the distribution of authority changes as this occurs. Gieryn states that:

' Boundary work occurs as people contend for, legitimate, or challenge the cognitive authority of science – and the credibility, prestige, power, and the material resources that attend such a privileged position. Pragmatic demarcations of science from non-science are driven by a social interest in claiming, expanding, protecting, monopolizing, usurping, denying or restricting the cognitive authority of science.'<sup>60</sup>

Gieryn suggests that when issues of epistemic authority arise, for example during controversies, people attempt to draw boundaries that allocate authority such that the debate is resolved one way or another. However, the activity of boundary work is interest driven. The groups or individuals that are in conflict attempt to draw boundaries such that they allocate themselves the required cultural authority to decide the resolution of any debate. As Sismondo has put it: 'When issues of epistemic authority arise, people attempt to draw boundaries: to have authority on a contentious issue requires that at least some other people do not have it.'<sup>61</sup> A group might attempt to extend the boundaries of their territory in order to gain extra authority to succeed in conflict, or a group might attempt to decrease or control the territory of another group to protect cultural authority already held. The boundaries are fluid, and are constantly being negotiated and redrawn through local and episodic contests. Authority is lost and gained by various groups during each contest, and with these changes in delineation of territory come changes in the cultural authority of the groups concerned.

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<sup>60</sup> Gieryn, T. (1995) p.405

<sup>61</sup> Sismondo, S. (2004) p.30



A popular tool for exploring the boundary work process is Thomas Gieryn's 'cartographic metaphor'.<sup>62</sup> Gieryn states that:

'The layered interpretations that surround scientists and scientific facts with a special believability often come in a rhetorical form best described as cartographic.'<sup>63</sup>

The metaphor allows that we understand society to be built up from numerous adjacent cultural territories, which fit together to form a cultural map. The 'inhabitants' of each territory are engaged in the maintenance of its perimeter, and in negotiation with the inhabitants of adjacent cultural spaces as to where exactly the boundaries that define the territories should be located. Individuals outside a particular territory can be as interested in the location of territorial boundaries as those 'inside'. Gieryn says that in light of the cartographic metaphor:

...'science' becomes a space on maps of culture, bounded off from other territories, labelled with landmarks showing travellers how and why it is different from regions of common sense, politics and mysticism.'<sup>64</sup>

He adds that, for scientists, the role of the cartographies is to:

...'provide interpretive grounds for accepting scientific accounts of reality as the most truthful or reliable among the promiscuously unscientific varieties always available.'<sup>65</sup>

Rhetoric is an important tool for the construction of these boundaries, and it is supported and reinforced by other signposts/landmarks that delineate territories. For example, in the case of the territory 'science', Gieryn says that the 'landmarks'

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<sup>62</sup> Gieryn, T. (1995)

<sup>63</sup> Gieryn, T. (1999a) p.x

<sup>64</sup> Gieryn, T. (1999a) p.x

<sup>65</sup> Gieryn, T. (1999a) p.x

include: 'white lab coats, laboratories, technical journals, norms of scientific practice, linear accelerators, statistical data, and expertise'.<sup>66</sup>

Gieryn suggests that four kinds of 'boundary work' activity contribute to the negotiation of cartographies: monopolization, expansion, expulsion and rejection.<sup>67</sup> These activities are conducted as part of a constant revision of the extent of the authority of the territory 'science'. They concern the ever-changeable delineation of the 'inside' from the 'outside'; that is, the delineation of those with the authority conferred by being inside science from those who have failed to secure that status and authority. This delineation results in the familiar process of marginalisation of some unorthodox individuals as they are removed from the inside and redefined as outsiders by drawing new boundaries. Boundary work is also the principle force behind the processes often termed professionalisation, in which a group attempts to redraw boundaries such as to allocate themselves cultural authority. From this point of view some conflicts might be expressed as boundary disputes between the occupants of adjacent territories.

Sociologists Harry Collins and Trevor Pinch have suggested that negotiation occurs in two key forums: the constitutive and the contingent.<sup>68</sup> It is useful to consider these forums as the arenas for boundary work. The constitutive is comprised of what might be considered the 'formal' spaces for scientific discourse and debate; the professional journals and professional conferences. The contingent forum is made up of all the other kinds of communication/interaction that seem to contribute to scientific debate; publication in popular media, personal communications, and e-mails. In each of these forums boundary work can be explicit or implicit.<sup>69</sup> Collins and Pinch argue that, in the case of implicit rejection, rival claims are simply ignored by orthodoxy, whereas, in the case of explicit rejection

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<sup>66</sup> Gieryn, T. (1999a) p.x

<sup>67</sup> Gieryn, T. (1995) p.424

<sup>68</sup> Collins, H. M. & Pinch, T. (1979)

<sup>69</sup> Collins & Pinch (1979)

there is usually a controversy in which the 'objects of dispute are articulated by individual scientists or opposed groups of scientists'.<sup>70</sup>

Some authors have explored more specific aspects of boundary work and boundary activity, in line with the general frameworks structured by authors such as Gieryn, Collins and Pinch. For example, Thomas Gieryn and Anne Figert have examined the role of fraud accusations in boundary work, with reference to the case of psychologist Sir Cyril Burt.<sup>71</sup> Also, Bruce Lewenstein has described the role of 'informal' publications and communications in the negotiation of unorthodox scientific claims through a study of the cold fusion debate.<sup>72</sup> Lewenstein's study provides an example of boundary work in the 'contingent' forum. In addition, French scientist Michel Schiff has presented a case study of 'rejection' in the constitutive and contingent forums in his study of the reception of Jacques Benveniste's 'water memory' theory.<sup>73</sup> Also, Collins and Pinch<sup>74</sup>, and separately Gieryn<sup>75</sup>, have studied experiment replication during the cold fusion controversy as a way of showing how a scientific conflict might be resolved without recourse to the kinds of proof or refutation that are commonly recognised as the mainstay of the scientific process. Policy researcher and sociologist Sheila Jasanoff has looked more broadly across the cultural cartography, and used the theory of boundary work to focus attention on the shifting associations between the adjacent territories 'science', 'politics' and 'policy'.<sup>76,77</sup>

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<sup>70</sup> Collins & Pinch (1979) p.239

<sup>71</sup> Gieryn, T. & Figert, A. E. (1986) Scientists protect their cognitive authority: The status degradation ceremony of Sir Cyril Burt. In: G. Bohme & N. Stehr (Eds.) *The knowledge society: The growing impact of scientific knowledge on social relations*. D. Reidel, Dordrecht.

<sup>72</sup> Lewenstein, B. (1995) From fax to facts: communication in the cold fusion saga. *Social Studies of Science*, 25: 403-436.

<sup>73</sup> Davenas, E., Benveniste, J. *et al.* (1988) Human basophil degranulation triggered by very dilute antiserum against IgE. *Nature*, 333: 816-818; Schiff, M. (1994) *The memory of water*. Thorsons (Harper Collins), London.

<sup>74</sup> Collins, H. & Pinch, T. (1993)

<sup>75</sup> Gieryn, T. (1992) The ballad of Pons and Fleischmann: Experiment and narrativity in the (un)making of cold fusion. In: E. McMullin (Ed.) *The social dimensions of science*. University of Notre Dame Press, Notre Dame, IN

<sup>76</sup> Jasanoff, S. (1987) Contested boundaries in policy relevant science. *Social studies of science*, 17(2): 195-230; Jasanoff, S. (1990) *The fifth branch: science advisors as policymakers*. Harvard University Press, London; Jasanoff, S. (1992) Science, politics and the renegotiation of expertise at

Gieryn's boundary work methodology has been a particularly important contribution to the sociology of scientific knowledge. As discussed above, Bloor identified symmetry as a key tenet of his 'strong programme', and boundary work methodology allows symmetry to be prioritised. Sociologists Brian Martin and Evelleen Richards have explained that, the symmetrical approach demands that 'the sociologist or historian must attempt to explain adherence to all beliefs about the natural world, whether they are perceived to be true or false, rational or irrational, successful or failed, in an equivalent or symmetrical way'. The analyst must also achieve this by examining 'both sides in the controversy using the same repertoire of conceptual tools'.<sup>78</sup>

Boundary work provides an excellent scheme by which to achieve this balance. It seeks boundary work activity on both sides of a conflict and describes the motivations of both advocates and adversaries using the same language, i.e. monopolization, expansion etc. It also refers to the ongoing process of boundary delineation, and so offers a scheme in which the categories right versus wrong and successful versus unsuccessful have less meaning or relevance, and instead the process of negotiation is prioritised above outcomes. Scott, Richards and Martin add that another demand of the symmetrical approach is that analysts should 'not grind an evaluative axe', and that 'if researchers are 'captured' by either side and become part of the debate, then they are deemed to have failed to maintain a symmetrical

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EPA. *Osiris*, 7: 1-23; Jasanoff, S. (1995) *Science at the bar: law, science and technology in America*. Harvard University Press, London.

<sup>77</sup> In addition to these explicit discussions of the boundary problem in science, Gieryn also suggests that further 'theoretical nourishment' might come from the studies of cultural boundary domains and delineations that occur in other areas of the cultural map than the scientific. (Gieryn, 1995:407) Gieryn shows that cultural boundaries have been explored in other contexts than the sociology of science. He cites four examples of other contributions to the boundary problem: i) the 'sociology of professions' in which the process of active professionalisation of trades and practices has been studied. ii) 'social worlds theory' which provides a descriptive tool analogous to cultural cartography for studying the boundaries between different 'subject sites' and jobs. iii) the 'history of cultural classifications' which uses the cartographic metaphor in detail to describe the means by which various cultural spaces have attained their classifications and boundaries. iv) 'feminist studies of science' in which boundaries have been studied in relation to their power to marginalize. (Gieryn, 1995)

<sup>78</sup> Martin, B. & Richards, E. (1995) Scientific knowledge, controversy, and public decision-making. In: S. Jasanoff, G. Markle, J. Petereson and T. Pinch (Eds.) *Handbook of science and technology studies*. Sage Publications, Thousand Oaks, CA. p.5

approach’.<sup>79</sup> Again, boundary work methodology allows analysts to avoid such asymmetry.

It is worth noting that these are potential benefits of using boundary methodology, but that, at the point of application of the methodology to case study analyses it is often difficult to make this potential count to its full extent. For example, section 1.6 describes how boundary methodology often ends up being used in the study of completed episodes of boundary activity, i.e. to examine the expulsion of a particular scientist from the cultural territory science. The result is that the potential of the methodology to eclipse the ‘successful versus unsuccessful’ and ‘right versus wrong’ considerations is not realised in full. In section 1.6 I argue that, at its point of use, boundary methodology often exhibits end-directedness despite its potential to avoid that asymmetry.

Some critics suggest that boundary studies have failed in the second part of the symmetry obligation; to avoid grinding an ‘evaluative axe.’ For example, as Scott, Richards and Martin describe, Collins and Pinch’s classic boundary work-study of the dispute over the existence of psychic phenomena has been criticised as an example of analysts being ‘captured’ by the debate.<sup>80</sup> Collins’ and Pinch’s analysis was taken up by the parapsychologists that they had studied, and used to support their case. Meanwhile, critics of parapsychology accused the sociologists of selective reporting. Mulkay, Potter and Yearley attribute this problematic outcome to Collins’ and Pinch’s uncritical adoption of the perspectives and terminology of the parapsychologists<sup>81</sup>; forcing a divide between researcher and researched is paramount in ongoing or contemporary conflict studies. So, while Gieryn’s methodology is potentially an excellent approach to the demand for symmetry, it often fails to achieve its full potential at the point of use. Sections 1.6 and 1.7 describe how this project aims to apply boundary work methodology in such a way as to promote the symmetry that it offers. In particular, in section 1.7, I argue that

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<sup>79</sup> Scott, P., Richards, E. & Martin, B. (1990) Captives of controversy: the myth of the neutral social researcher in contemporary scientific controversies. *Science, Technology and Human Values*, 15(4): 474-494.

<sup>80</sup> Scott, P., Richards, E. & Martin, B. (1990)

<sup>81</sup> Mulkay, M., Potter, J. & Yearley, S. (1983)

my concept of 'perpetuating forces' promotes symmetry when combined with boundary work analysis.

This is by no means an exhaustive review of attention to the 'demarcation' or 'boundary' problems. I have dealt with some key positivist and relativist approaches, since these are the most relevant to the combined methodology used in this project. There have of course been many other contributions, of equal merit to those laid out here, from both the essentialist and constructivist schools of thought. For example, C. P. Snow famously tackled the issue of the demarcation of science in his description of the 'two cultures'.<sup>82</sup> Controversy has also been studied from the perspective of both 'group politics' and 'social structuralism'.<sup>83</sup> Martin and Richards state that, from the group politics perspective, 'controversy is dealt with as any other form of politics in the pluralistic interpretation of liberal democracy: a process of conflict and compromise involving various groups contending in a political marketplace'.<sup>84</sup> They suggest that the group politics approach is best suited to controversies in which there are obvious contending groups, for example, the public versus government. This approach expresses the behaviours of participants in relation to the motivations of the interested group to which they belong. For example, Dorothy Nelkin has considered the nuclear power debate from this perspective, examining the interaction of government, public and science.<sup>85</sup> Furthermore, the social structuralist approach uses 'concepts of social structure, such as class, the state, and patriarchy, to analyse society and to provide insights into controversial issues'.<sup>86</sup> For example, that approach underlies treatments that would be classified as feminist or Marxist, i.e. sociologist Gena Corea has offered feminist analysis of the reproductive technologies controversy.<sup>87</sup>

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<sup>82</sup> Snow, C. P. (1959) *The two cultures and the scientific revolution*. Cambridge University Press, Cambridge

<sup>83</sup> Martin, B. & Richards, E. (1995)

<sup>84</sup> Martin, B. & Richards, E. (1995) p.4

<sup>85</sup> Nelkin, D. (1971) *Nuclear power and its critics: The Cayuga Lake controversy*. Cornell University Press, New York.

<sup>86</sup> Martin, B. & Richards, E. (1995) p.5

<sup>87</sup> Corea, G. (1985) *The mother machine: reproductive technologies from artificial insemination to artificial wombs*. Harper and Row, New York; Corea, G. et al (Eds.) (1985) *Man made women: How new reproductive technologies affect women*. Hutchinson, London.

In summary, this review seeks to achieve two goals. Firstly, it introduces some of the approaches and methodologies that will be applied in this analysis. Secondly, it illustrates that there has been some transition from essentialist to constructivist approaches to the study of science. I argue below that, although some of the essentialist tools have been heavily criticised, certain of their elements might be applied alongside the newer constructivist approaches to achieve a richer analysis. Like the internalist/externalist division, the essentialist/constructivist division is an artificial one. To reject tools from one tradition, in favour of those from the other, is thus potentially wasteful and short sighted. I propose a synthetic methodology for the study of controversy in this project, using language and analytical tools drawn from both of these traditions. Sections 1.2 and 1.3 examine in more detail the particular tools and theories, both essentialist and constructivist, that are applied in combination to enable my particular analytical approach.

## **1.2 A synthetic methodology 1: How philosophical approaches to the ‘demarcation problem’ might illuminate the directed mutation debate and the anatomy of protracted controversy.**

As described above, the philosophical tools that address the structure and dynamics of scientific conflict are generally essentialist in their approach; that is they seek to describe scientific activity in terms of the intrinsic qualities that delineate it from other kinds of knowledge-producing or creative work. Some more recent sociological approaches shift attention from the ‘demarcation problem’ to the ‘boundary problem’, and shift emphasis from essentialist to constructivist treatments. In this section, I argue that some tools from the philosophical essentialist tradition might remain useful to the constructivist study of scientific conflict, if used in abstraction from their essentialist view of science; allowing essentialist tools to be put to use without invoking the essentialist critique. In particular, some essentialist philosophical models provide valuable language for exploring the structure and

dynamics of scientific conflict. This section considers those models and their utility in this project.

### i) The Kuhnian Paradigm

In general, boundary theory assumes that contests are local and episodic, and that during them authority is open to review. It does not deal expressly with the defence of orthodoxy that underlies negotiation in some debates; that is, it does not allow for contests in which authority is less clearly open to review. Thomas Gieryn notes the possible existence of stable territories that persist between episodes of mapping. He suggests that in some cases 'cartographies get stabilized as unquestioned tacit assumptions or as uncontested old maps'<sup>88</sup>, and that these 'old maps' should be studied in greater detail as a contribution to the development of boundary theory. In this project I explore how stabilized authority can be created through a series of contests; examining the genesis and perpetuation of 'old maps' in relation to the principal case study. I argue that where old maps exist subsequent debates are not local and episodic, and that authority is not always open to review. Rather, a consensus regarding authority emerges as orthodoxy and is defended against review.

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In this project I use the language of Kuhn's paradigm theory to explore the emergence of consensus regarding authority, and the defence of orthodoxy that protects it. I suggest that Kuhn's notion of paradigm and paradigm defence can provide the language for exploring these issues, showing how some groups come to monopolize authority and seek to protect that monopoly between contests. Applied as a rhetorical tool, Kuhn's theory can be put to work without invoking the problems

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<sup>88</sup> Gieryn (1999a) p.34-35.

<sup>89</sup> Boundary work theory suggests that during each conflict territories are redrawn afresh, locally and contingently. Therefore there is no real sense in speaking of a mainstream or establishment, or of the defence of 'existing' territory. The theory does not accept a priori categories, which is one aspect that contributes significantly to its high degree of symmetry. However, it is important to note that Gieryn does allow in some circumstances for the pervasive influence of what he calls 'a cartographic legacy'. This refers to the persistence between mapping episodes of certain territories that become more stable and easily redrawn as the result of 'accumulated residues of previous instances of boundary work'. These cartographic legacies result in certain territories becoming 'sedimented as an ordinary tacit space on most everybody's mental map of culture' such that in some instances 'creased and dog-eared maps' are unfolded and re-used rather than drawn afresh. (Gieryn, 1999 p.19-20) In that light it becomes more reasonable to speak of a mainstream and their orthodoxy.



conferred by the theory's essentialism. In chapter 3, I argue that Gieryn's 'old map' is roughly analogous to Kuhn's 'paradigm'. I suggest that it might be useful to invoke the familiar concepts of the 'paradigm' and 'paradigm defence' to describe the community dynamics that enable the kind of stability between mapping episodes that Gieryn has highlighted for investigation.

In addition, in Chapter 4, I contrast Kuhn's paradigm theory and Lakatos' research programmes model to illustrate the different approaches of the groups involved in the case study debate. I suggest that, depending on the group's interests, they adopt either a more Kuhnian defence of consensus<sup>90</sup> or a more Lakatosian approach to anomaly<sup>91</sup>. I argue that the Kuhnian approach promotes the preservation of old maps, whereas the Lakatosian promotes the local and episodic nature of contests. Contrasting participants' approaches in this way helps reveal goals and motivations, and illustrates groups' differing perceptions of appropriate closure.

#### ii) The Mertonian 'norms'

The language of Merton's normative scheme<sup>92</sup> is put to work in this project to further reveal the role of 'old maps', and illuminate the defence of orthodoxy that allows their operation. Merton's norms imply responsibilities, which if adhered to would both protect the local and episodic nature of contests, and the openness of authority to review. For example, Merton's scheme requires 'organized scepticism', meaning a withholding of judgements on new findings until such time as they have been evaluated scientifically. In that light, each anomaly is dealt with on the basis of its own merit, and the outcome of any contest is self contained and not determined by existing assumptions.

However, in cases where old maps persist and orthodoxy is defended the obligations of Merton's norms are not met. Disinterestedness and scepticism are quickly abandoned in service to defence of an old map. In this project, participants' actions are reviewed in relation to their adherence to the norms. Where the norms

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<sup>90</sup> As expressed in: Kuhn, T. (1962)

<sup>91</sup> As expressed in: Lakatos, I. (1970) Falsification and the methodology of research programmes. In : I. Lakatos & A. Musgrave (Eds.) *Criticism and the growth of knowledge*. Cambridge University Press, Cambridge; Lakatos, I (1976) *Proofs and refutations*. Cambridge University Press, Cambridge.

<sup>92</sup> As expressed in: Merton, R. K. (1942)

are subverted, the goals and interests of participants are made clearer. For example, where organized scepticism is abandoned the interests of a group in preserving an existing authority allocation are revealed. In this project the action of groups and individuals is evaluated in relation to the imperative of the norms, and abandonment of one or more of the norms is identified as an outcome of the creation and perpetuation of an old map.

In addition, I use the language of Merton's normative scheme to evaluate the contributions of some key authors in the case study debate. During conflicts contestants often use the rhetorical device of accusing their opponents of unorthodoxy, or even 'pathological' science. Without using Mertonian language, they are nevertheless often appealing to the responsibilities of disinterestedness and communism in their accusations. In Chapter 4 I explore the relationship between subversion of the norms, and the branding 'unorthodox', and consider whether there has been 'pathological' scientific activity in the case study debate.

### iii) Popper's falsification concept

In cases where old maps are being used to validate persistent authority allocations there is a degree to which the authority allocation is adopted as an object of faith; the usual processes of proof and refutation are eclipsed by the defence of the faith object. Recourse to scientific detail in resolution of conflicts is diluted by reference to the details of the old map and the authority allocation it recommends. Where aspects of scientific knowledge begin to be defended using old maps, falsifiability is lost.

In this project I use Popper's falsification concept to assess the degree to which old maps recommend the adoption of unfalsifiable faith objects. I argue that, in order to preserve an old map, falsifiability must be sacrificed by some interested parties. In Chapter 5, I describe the tacit popular view of falsification as one mark of objectivity in science, and illustrate how adherence to old maps defies the public identity of science.

So, essentialist theories are used here to contribute useful visual aids and language for the analysis of particular elements of the case study, and as such, these models can bolster the constructivist tools in attempts to uncover motivation and goals in particular groups of controversy participants. They provide a clear, and often familiar, language by which to express some of the participants' behaviours. The theories' essentialism need not figure explicitly, but rather they can be put to work in the service of a largely constructivist analysis.

Essentialist philosophical approaches are also often useful for discussion of popular engagement with scientific conflicts, since essentialism remains dominant in popular perceptions of science, in spite of academics' enthusiasm for constructivist approaches. In addition, the scientific community has often taken an essentialist approach to its own self-definition<sup>93</sup>; scientists often subscribe to the idea that they are engaged in an activity that merits special status on account of intrinsic features of its methodology and practice. In line with this self-styling, the scientific response to unorthodoxy is often convened in line with the terms of the essentialist approach. That is, unorthodox individuals or groups are often portrayed as being in some way 'unscientific' or 'pathological', and that alone appears as evidence by which to disregard their work. So, in spite of the criticism of essentialism within science studies, that perspective remains embedded in scientific practice and the engagement of the public with that practice. Thus, as this project illustrates, the essentialist tools retain the capacity to illuminate some of the action of scientific conflicts.

### **1.3 Synthetic methodology 2: How sociological and constructivist approaches to the 'boundary problem' might illuminate the directed mutation debate and the anatomy of protracted controversy.**

Thomas Gieryn's 'boundary' theory<sup>94</sup> examines the negotiation and allocation of epistemic authority during episodes of conflict, and describes how participants' contribute to and influence that process through 'boundary work'

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<sup>93</sup> Gregory, J. (1998) *Fred Hoyle and the popularisation of cosmology*. PhD thesis, Imperial College of science, technology and medicine.

<sup>94</sup> Gieryn, T. (1983)

activities. His concepts provide perhaps the most broadly useful methodological tool for exploring the circumstances of the directed mutation debate and revealing the interests of the participants.

The boundary work concept facilitates three analytical tasks in this project. Firstly, it reveals the activity of advocates and adversaries as they compete for authority. Boundary theory allows that contest to be framed as a struggle for epistemic authority between two principal groups: one which wishes to retain its epistemic authority and the other which wishes to challenge that authority and win a share of it. Boundary work theory makes apparent the goals and motivations underlying each group's activities and rhetorical devices. It frames their activity as boundary work for expansion (the advocates) versus protection and expulsion (their adversaries), and illuminates the struggle for authority across the inter-disciplinary boundary.

Secondly, the concept of boundary work is invaluable in the discussion of the broader intellectual and social context of the directed mutation debate. It allows the directed mutation debate to be viewed as an episodic contest within the greater and longer-term authority struggle between competing programmes. This reveals elements of the participants' boundary work as serving their broader, though less explicit, interests: the acceptance versus rejection of their wider agendas.

Thirdly, boundary theory and the cartographic metaphor provide a useful, and much needed tool for the analysis of the somewhat murky structure and dynamics of the Internet based component of the debate. In relation to this aspect of the debate, boundary theory allows us to ask some useful questions regarding this context for conflict, for example: 'how are boundaries identified in the virtual world?'; 'does conflict negotiation online contribute to the negotiation of cultural cartographies?'; 'does boundary work still function in the Internet forum?' and 'do authority allocations break down without explicit cartography and authority signifiers?'.

Three of Gieryn's boundary work categories - monopolization, expansion and expulsion - are particularly useful:

i) Monopolization: Gieryn describes this kind of boundary work as ‘a cartographic contest for cultural authority’<sup>95</sup>, whereby, a particular group asserts the dominance of their programme, and drives the exclusion of other groups such that they gain sole control over epistemic territory and garner authority for themselves alone. The boundaries of that territory are then policed heavily against the assaults of other groups who wish to take or share the authority. Gieryn<sup>96</sup> suggests that this kind of boundary work is exemplified by the case of the seventeenth century epistemic conflict between Hobbes and Boyle that historians Steven Shapin and Simon Schaffer<sup>97</sup> have analysed. In that conflict both Hobbes and Boyle engaged in boundary work designed to take control of *all* the cognitive authority attached to the territory currently defined as ‘science’; both stated that their particular epistemological approach best ensured the success and progress of science. There was no potential for heterogeneity or for the territory to be shared; monopolization was the goal of each of their activities.

In Chapter 3 I describe the rise of Darwinian evolutionary biology as a process of progressive authority monopolization. I illustrate how the monopoly was created, recorded as an old map, and later deployed in subsequent debates to influence authority allocations.

ii) Expansion: In this category Gieryn groups all those boundary work activities that occur when ‘insiders seek to push out the frontiers of their cultural authority into spaces already claimed by others’.<sup>98</sup> As an example of this kind of activity he cites the work of the eighteenth century *philosophes*, who sought to extend their ‘mixture of rationalism and empiricism into a domain of questions and problems owned by religion and embodied in the institution and dogma of the church’.

The principal case study examined in this project involves an interdisciplinary contest for authority. The concept of expansion enables a clearer understanding of that aspect of the contest; revealing how the participants struggled

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<sup>95</sup> Gieryn, T. (1995) p.424

<sup>96</sup> Gieryn, T. (1995)

<sup>97</sup> Shapin, S. & Schaffer, S. (1985)

<sup>98</sup> Gieryn, T. (1995) p.429

either to expand their authority into a territory held by another discipline, or defend the territory held from that expansion.

iii) Expulsion: Gieryn suggests that expulsion is a common goal of boundary work.<sup>99</sup> He defines expulsion as the act of ‘insiders’ removing ‘not-real’ members from their midst; this is achieved by labelling the rejected individuals as; ‘deviant, pseudoscientist, amateur, fake’.<sup>100</sup> Those who are expelled are accused of exploiting the authority that should only be due to those who are accepted into the consensus, and thus ‘inside’, of the scientific territory. Such individuals may appear as real scientists, but they have not earned the cognitive authority they claim. Gieryn suggests that the 1970s fraud accusations against Sir Cyril Burt provide an example of boundary work to affect expulsion.<sup>101</sup> Thomas Gieryn and Anne Figert have studied Burt’s case, and have analysed the process by which his expulsion from the ‘inside’ was achieved through accusations of fraud.<sup>102</sup>

In this project I consider the role of expulsion in the perpetuation of old maps. I argue that expelling opponents of a stable authority allocation prevents review of epistemic authority and that, therefore, expulsion is a goal of individuals who are interested in preserving the *status quo*. I describe how expulsion is a necessary part of the process of progressive authority monopolization. I illustrate that there are local examples of expulsion within the case study, as well as a significant legacy of expulsion underlying the old map that influenced this apparently local debate. I argue that the old map that influenced the case study debate was formed in part through accumulated instances of expulsion.

Overall, boundary theory is useful in this project as a tool or revealing interests and motivations. Understanding the ongoing nature of the struggle for epistemic authority reveals both the local conditions of the case study, the stakes of the interdisciplinary aspect of the debate, the means by which old maps are drawn,

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<sup>99</sup> Gieryn, T. (1995)

<sup>100</sup> Gieryn, T. (1995) p.432

<sup>101</sup> Gieryn, T. (1995)

<sup>102</sup> Gieryn, T. & Figert, A. (1986)

and the legacies of the old maps as they are unfurled in new contests. Additionally, boundary theory provides a revealing tool for discourse analysis (important in the construction of the narrative case study from the primary journal articles), allowing rhetoric to be understood as boundary work for the achievement of expansion or protection, and allowing expulsion to be seen as a tool for defending authority.

#### Some additions/extensions to boundary work theory

Thomas Gieryn has noted two areas of deficiency in existing boundary theory and highlighted these for further work.

i) Boundary work gives ‘greater attention to boundaries between science and something else altogether than those dividing the various sciences and scholarly disciplines’.<sup>103</sup>

ii) ‘...too little is said about when and how some cartographies get stabilized as unquestioned tacit assumptions or as uncontested old maps...’

In this project I address these issues, pursuing Gieryn’s recommendations for further work, and seeking to increase the utility of boundary theory for this analysis.

i) I explore the possibility of extending the language of boundary work to describe the shifting interdisciplinary boundaries that delineate the space within the cultural territory ‘science’. In Chapter 4, I use Gieryn’s categories of boundary work (expulsion, expansion and monopolization) to describe the motivations and activities that determine the debate between the sub-disciplines. I aim to demonstrate that the same tools used by scientists to define their cultural domain are also used within that domain to manage the sharing of territory by the disciplinary groups. In Chapter 4, I describe the extent to which the authority (or amount of territory) allotted to the groups within the overall cultural territory ‘science’ is equally as local, episodic and

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<sup>103</sup> Gieryn, T. (1999) p.34

changeable as the boundary of the territory 'science' itself. I suggest that a larger-scale map is required to understand the detailed activity of scientific conflict. Contests concerning the boundaries of the cultural space 'science' explain some of the rhetoric and activity of its practitioners, but there is also a great deal of activity aimed at drawing and shifting the internal boundaries.

ii) In Chapter 3, I explore the persistence of certain cultural territories between mapping episodes. I use a historical account to exemplify how a territory can become stabilized. I describe how boundary work tactics, which usually help construct local and transient boundaries, can be used to create a more durable delineation. I argue that cultural authority can be allocated not just as the outcome of one boundary contest, but rather as the cumulative legacy of many contests. I use Gieryn's language of 'old maps' to show how authority can be transferred more or less intact between conflict episodes, illustrating how a territory can be created and then deployed so as to retain its authority in a variety of different cartographies. I show that authority accumulated across many contests leads to the development of 'tacit assumptions' regarding that authority within the cultural cartography. Through the circumstances of the principal case study I explore the 'accumulated residues' that Gieryn suggests contribute to these kinds of persistent cultural territories.

#### **1.4 Looking at scientific controversy from different perspectives.**

Of course, not all discussions of scientific conflict are so explicitly motivated by attention to the 'demarcation' or 'boundary' problems. For example, there are also controversy studies and theories focussed more specifically on policy-making, legislation, politics and economics. In this project, these kinds of treatments are less broadly useful in methodological terms. They are less concerned with the internal structure and dynamics of scientific conflicts and the behaviour of advocates and adversaries, and largely focus on the specifics of the interaction of science with external forces such as government and funding, often emphasising issues of ethics



or finance, which are largely beyond the scope of this analysis. However, despite that quite different emphasis some elements can be drawn from that literature to add value to this analysis.

A case in point is the Closure Project carried out from 1978 to 1982. The project was funded by the U.S National Endowment for the Humanities to address the character of ‘scientific disputes with a heavy ethical or political overlay’.<sup>104</sup> The outcome of the project was an edited collection of case studies exploring the influence of ethical or political factors on the structure and dynamics of scientific conflicts. Although the project’s conclusions concerning the interplay of politics, ethics and science do not help illuminate the directed mutation debate (or the analogous cases examined here), the Closure Project does provide precedent for one of the key approaches in this project. In section 1.7, I argue that a useful perspective for achieving a clear picture of negotiation in protracted scientific controversies is to think in terms of the active ‘perpetuating forces’ that have contributed to the extension of negotiation. In the Closure Project a similar approach is evident. In that project certain aspects of the debates studied are considered as ‘impediments’ to closure of the conflict.

The authors of the Closure Project collectively establish that the existence of moral or ethical elements in a debate will serve to impede the standard processes of closure (i.e. appeal to further evidence, argument, abandonment, circumscription).<sup>105</sup> They achieved this by using examples drawn from four principal case studies; the use of Laetrile in cancer treatment, the classification of homosexuality as a disease, the assessment of risks in the workplace, and the safety of nuclear power. The authors show that the negotiation of these debates has by no means been confined to matters of empirical fact or epistemology. Rather, the path to resolution has been shaped by financial and political interest and by moral and ethical imperatives; reason and objectivity have been overlaid by interests, feelings and money.

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<sup>104</sup> Engelhardt, T. (Jr) & Caplan, A. (Eds.)(1987) *Scientific controversies: Case studies in the resolution and closure of disputes in science and technology*. Cambridge University Press, New York.

<sup>105</sup> Engelhardt, T. (Jr.) & Caplan, A. (Eds.) (1987)

Without using the language of controversy ‘perpetuation’ that I adopt here, the authors are nevertheless identifying moral and ethical ingredients as forces of conflict protraction. The Closure Project therefore offers a precedent for the study of the active elements of protracted controversy in apparently unresolved or unresolvable conflicts. It is such an approach to controversy closure that this project aims to elaborate upon. In this analysis I seek further examples of how particular aspects of a debate might contribute actively to its protraction and might determine the quality of the negotiation phase. The identification of a selection of these forces is the unifying theme of Chapters 3, 4 and 5.

Treatments of scientific controversy also exist in the form of the numerous, and often familiar, popular accounts of scientific conflict. These largely take the form of historical accounts of particular episodes of conflict and aim only to present that history as a narrative or ‘story’, rather than as an analysis. In some cases, introductory or concluding passages may offer statements concerning scientific controversy more generally, but these statements have a secondary role in relation to main function of these texts, which is most often entertainment. Even the general statements that these texts include are often useless to an analysis such as this one, since they betray, incorporate or pander to various simplistic views of the nature of scientific controversy. These treatments of controversy are often sensationalist, a tendency that is fostered by their entertainment role. They also often rely upon certain unarticulated interpretations of conflict as their premise, for example, that conflict represents ‘pathological science’, that controversy is a perversion of the scientific process and that scientists who engage in controversy are invariably motivated by self-interest. The materials in this category are therefore omitted from this analysis.

### **1.5 Scientific controversies, historical narrative, and the role of case studies in the development of analytical tools. Justifying the case study-based approach.**

The study of scientific controversy is often linked with case study-based methodology. In the categories of literature described above case studies are deployed in a variety of ways. In some of the sociological and philosophical treatments case studies are referred to with minimal detail; they are invoked to illustrate theoretical points. For example, Gieryn<sup>106</sup> outlines his categories of boundary work and illustrates each of them with reference to a case study that has been developed by another author. For example, he argues that the boundary work process of 'expansion' is illustrated in Robert Darnton's study of D'Alembert's enlightenment project<sup>107</sup> and that 'monopolization' is demonstrated in Shapin and Schaffer's study of the seventeenth century conflict between Hobbes and Boyle.<sup>108</sup>

In other sociological and philosophical treatments single case studies are developed in detail by the authors as a means of illustrating several theoretical points using a single example. This methodology underlies Gieryn and Figert's study of fraud and 'expulsion' in relation to the case of Sir Cyril Burt.<sup>109</sup> That case study is constructed in such detail that the authors have essentially written a paper 'about Burt' as much as they have written a paper 'about controversy'. This same trend is also reflected in; Michel Schiff's analysis of exclusion with close reference to Jacques Benveniste's 'water memory',<sup>110</sup> Harry Collins' study of the replication of controversial research findings with reference to the detection of gravitational waves<sup>111</sup>, Trevor Pinch's study of conflict sociology with reference the neutrino detection debate<sup>112</sup>, Martin Rudwick's study of the shaping of scientific knowledge with reference to 'the great Devonian controversy'<sup>113</sup> and Bruce Lewenstein's study of publication during conflicts with reference to the Cold Fusion controversy.<sup>114</sup>

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<sup>106</sup> Gieryn, T. (1995)

<sup>107</sup> Darnton, R. (1984) *The great cat massacre*. Basic Books, New York .

<sup>108</sup> Shapin, S. & Schaffer, S. (1985)

<sup>109</sup> Gieryn, T. F. & Figert, A. (1986)

<sup>110</sup> Schiff, M. (1994)

<sup>111</sup> Collins, H. M. (1985)

<sup>112</sup> Pinch, T. (1986) *Confronting nature: the sociology of neutrino detection*. Reidel, Dordrecht, the Netherlands.

<sup>113</sup> Rudwick, M. (1985) *The great Devonian controversy: The shaping of scientific knowledge among the gentlemanly specialists*. University of Chicago Press, Chicago.

<sup>114</sup> Lewenstein, B. (1995)

In some other analytical controversy studies a series of cases are presented to illustrate a particular theoretical point. This methodology is exemplified in the Closure Project in which four cases studies are developed to illustrate the thesis that moral and ethical elements in controversies impede the processes of closure.<sup>115</sup>

In popular accounts of scientific controversy the development of case studies becomes the priority and analysis is eclipsed by this shift of attention towards self-contained narratives that constitute 'stories' of scientific controversy. Some of these accounts retain a degree of analytical voice, while others abandon theory or generality altogether in the pursuit of 'entertainment'. Examples of this kind of treatment include Hal Hellman's *Great Feuds in Science*<sup>116</sup>, Michael White's *Rivals: conflict as the fuel of science*<sup>117</sup> and Arthur Koestler's *The Case of the Midwife Toad*.<sup>118</sup>

Therefore, the case study based approach of this project follows precedent for the study of controversy. A detailed narrative describing the directed mutation debate is developed in this project as a means by which to illustrate the theoretical issues that are addressed and analysed. That principle case study is presented in Chapter 2, and in Chapters 3-5 various technical or theoretical issues are explored in relation to that material. Supporting case studies are included throughout to demonstrate the generality of many of the theses that I explore.

Further justification for the use of the case study-based approach can be drawn from Martin Rudwick's explicit statements concerning the role of narrative in methodologies for the study of conflict. Rudwick states that:

'It is high time for a genuine revival of narrative to be set in train, but it must be narrative with a purpose, and no mere chronicle. In the fine-grained study of scientific research practice, narrative is not so much a literary convenience as a methodological necessity. If

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<sup>115</sup> Engelhardt, T. (Jr.) & Caplan, A. (Eds.)(1987)

<sup>116</sup> Hellman, H. (1998)

<sup>117</sup> White, M. (2001) *Rivals: conflict as the fuel of science*. Secher & Warburg, London.

<sup>118</sup> Koestler, A. (1971) *The case of the midwife toad*. Random House, London.

scientific knowledge is to be studied *in the making*, the closest attention must be paid to strict chronology, not only in description but also in analysis.’<sup>119</sup>

Rudwick contends that, to understand the activities and motivations of individuals involved in establishing new scientific knowledge claims, the process that they experience must be re-created. The intricate aspects of the pursuit of knowledge and the interactions of individuals must be discovered if a realistic portrayal of the scientific enterprise is to be achieved. Rudwick advocates the use of detailed narrative as a tool by which to accomplish this closeness to the object of science studies. He adds that:

‘A narrative that does justice to the twists and turns of research does indeed need to be long and detailed.’<sup>120</sup>

To follow the spirit of Rudwick’s recommendation of narrative as a central methodology for science studies, and in line with his advice as to the proper length of such a narrative reconstruction, this project is case study-based and that case study is developed in detail (Chapter 2). Chapter 2 provides the ‘story’ of the directed mutation debate (which is one desired outcome of controversy studies ie. in popular treatments or at the foundation of discourse analysis) but also goes beyond being ‘mere chronicle’ by acting as the basis for a general exploration of scientific controversy themes (as is the desired outcome of the more technical treatments of controversy).

## **1.6 Enthusiasm for the ‘ends’ of conflicts and the neglect of protracted negotiation**

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<sup>119</sup> Rudwick, M. (1985) p.11

<sup>120</sup> Rudwick, M. (1985) p.12

As discussed above, descriptive and analytical tools concerning the structure and dynamics of scientific conflict are abundant. These tools, and the approaches that they embody, appear diverse and varied. One might loosely categorize them in terms of their focus as philosophical, sociological or historical, as I have done here for evaluative clarity. Despite their variety, the majority of these approaches are united by their similar assumptions regarding the anatomy of scientific conflict. In this section I describe the limitations of those assumptions and their influence on the utility of technical resources for the study of protracted controversy.

There is a common structure inherent in the majority of theoretical models of scientific conflict; the vast majority of descriptive and analytical tools assume that a three-part process underlies controversy activity. In spite of their specific terminology or depth of focus, what each model effectively describes is the process by which an **anomaly arises**, is **negotiated** and is **resolved**. In general, these descriptions and analyses, whether they are predominantly philosophical, sociological or historical, are invariably focussed upon the achievement of resolution and closure. Even where description of the means of anomaly resolution is not the primary aim, still theoretical tools assume closure as the ultimate goal or end point of scientific conflict; they focus on the processes by which anomalies are rendered unproblematic. Even those sociological models concerned with the activity of controversy participants are directed to describe how participants resolve their differences. The sociological theories, particularly the constructivist approaches, take better account of interest and motivation than the philosophical and historical, but they are still structured around the notion that disputes have an ultimate end. The effect of this common approach is similar to the problem of present-centredness.<sup>121</sup> There is a temptation to analyse controversy teleologically.

Even in the case of boundary work study, where the process of anomaly identification, negotiation and closure is most blurred, and there is explicit attention

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<sup>121</sup> Ashplant & Wilson see the problem of present-centredness arising when ‘...the historian, in seeking to study, reconstruct and write about the past, is constrained by necessarily starting from the perceptual and conceptual categories of the present. [Ashplant, T & Wilson, A. (1988a) Present-centred history and the problem of historical knowledge. *The Historical Journal*, 31:253-274. p.253] See also: Ashplant, T & Wilson, A. (1988b) Whig history and present-centred history. *The Historical Journal*, 31: 1-16.

focussed on the motivation of boundary workers and the ongoing nature of contests, still the aims and outcomes of the actors' tactics reflect some kind of closure. Any one of the cartographies that are constantly being locally and episodically generated represents the end of a particular contest. As the territories expand and contract some groups might be excluded or assimilated - but the goal/outcome is still a community cohered by a common consensus. Where the boundary work concept is applied to real cases it is often to elucidate historically completed acts of 'exclusion' or 'monopolization'.<sup>122</sup> So, while boundary work theory is in no way expressly or necessarily teleological or present-centred, it is often applied as a methodology to situations that imply end-directed outcomes of boundary activities.

It is unsurprising that the closure of controversy is all-important in theoretical frameworks; closure appears to be the goal of scientific activity – it is portrayed and often acknowledged as the motivation for those involved in a controversial episode. This clear enthusiasm for the 'ends' of conflicts can be attributed as a function of two legacies: (i) the self-image and public image of the scientific endeavour, and (ii) the predominance of analyses of scientific knowledge over analyses of scientific activity:

i) Controversy is an undesirable activity in science, often perceived as failure of the scientific process. Philosophers of science Peter Machamer, Marcello Pera and Aristides Baltas describe aversion to conflict, stating that: '...the absence or resolution of disputes has been taken as the hallmark of scientific knowledge as compared to other disciplines or fields of experience'<sup>123</sup>, and that, the 'view that science should be ultimately uncontroversial flourished at the time of the founders of modern science' and has been perpetuated by scientists and philosophers ever since.<sup>124</sup>

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<sup>122</sup> For example Gieryn (1999a) acknowledges that the five examples of boundary work that he explores are '...all historical rather than contemporary, more or less resolved rather than ongoing contests for credibility.' p.34. Also, Gieryn (1995) describes the complete act of Sir Cyril Burt's 'status degradation', as do Gieryn and Figert (1986) and Gieryn (1992) describes the 'unmaking' of Cold Fusion.

<sup>123</sup> Machamer, P., Pera, M. & Baltas, A. (2000) *Scientific Controversies: Philosophical and Historical Perspectives*. Oxford University Press, New York. p.4.

<sup>124</sup> Machamer, P., Pera, M. & Baltas, A. (2000)

Karl Popper stated that: 'Science is one of the very few human activities, perhaps, the only one, in which errors are systematically criticised and often eventually corrected'.<sup>125</sup> Machamer, Pera and Baltas add that it is that conception of the process of science that leads to the state in which: '...it is typical of scientists and others to believe that any dispute or controversy is resolvable given further information or data, which, in their turn, necessarily will be forthcoming given enough time and money.'<sup>126</sup> Therefore, the very nature of the scientific enterprise pre-supposes closure.

As Dorothy Nelkin suggests: 'The authority of scientific expertise rests on assumptions about scientific rationality'.<sup>127</sup> Effectively, 'science is judged rational because it is based on data collected through rational procedures.'<sup>128</sup> Controversy appears to undermine or challenge that rationality. Controversy analysts Tristram Engelhardt and Arthur Caplan state that: 'Contemporary societies take science seriously. They presume that science can resolve factual issues...'.<sup>129</sup> They add that: 'Also, much of Western public policy has presupposed that scientific controversies are resolvable by rational analysis and the investigation of the facts. Science has been presumed to be objective.'<sup>130</sup> Machamer, Pera and Baltas suggest that there is a 'paradoxical dissociation between science as actually practiced and science as perceived or depicted by both scientists and philosophers', and that, 'while nobody would deny that science in the making has been replete with controversies, the same people often depict its essence or end product as free from disputes, as the uncontroversial rational human endeavour par excellence.'<sup>131</sup>

These commentaries illustrate the extent to which the widespread identity of science incorporates, and indeed relies upon, an element of trust. Trust relates to the expectation that science will operate an objective system by which appropriate

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<sup>125</sup> Popper, K. (1963) p.216

<sup>126</sup> Machamer, P., Pera, M. & Baltas, A. (2000) p.4

<sup>127</sup> Nelkin, D. (1982) Controversy as a political challenge. In: B.Barnes & D. Edge (Eds.) *Science in context: Readings in the sociology of science*. Open University Press, Milton Keynes.

<sup>128</sup> Barnes, B. & Edge, D. (Eds.) (1982) *Science in context: readings in the sociology of science*. Open University Press, Milton Keynes.

<sup>129</sup> Engelhardt, T. (Jr.) & Caplan, A. (Eds.)(1987) p.vii

<sup>130</sup> Engelhardt, T. (Jr.) & Caplan, A. (Eds.)(1987) p.3

<sup>131</sup> Machamer, P., Pera, M. & Baltas, A. (2000) p.3



resolution of problems might certainly be achieved. The ability of science to achieve such resolutions earns it regard as a special kind of activity. It appears that the resolutions achieved in science can be trusted on account of the method that has been applied in their achievement. Where resolution seems problematic or unachievable, then trust in the method is effectively betrayed. What should be a failsafe methodology begins to appear imperfect. Any indication of such failure is inevitably problematic in 'a culture that looks to scientific investigations, panels, and commissions for the determination of facts and the resolution of scientific disputes.'<sup>132</sup>

It seems that protracted debates in science undermine the 'special' or dignified nature that has been attributed to the scientific enterprise and the knowledge that it generates. They disappoint the view of science that assumes a perfect methodology is in action that cannot result in anything other than the progressive and inevitable generation of 'facts' or more secure knowledge claims. Protracted debates undermine the image of science perpetuated from within the profession itself, and clash with the perception of science that has been adopted by non-scientists. The ends of conflicts thus become a focus; resolution reaffirms the image of science upon which trust and authority is based.

ii) Engelhardt and Caplan contend that: 'For some time the analysis of science by philosophers, sociologists, historians and others has been dominated by discussions about theory change and development. Metascientific studies have not progressed far beyond bitter wrangles as to the adequacy of such concepts as 'paradigms', 'research traditions', 'themata' and 'theories' for adequately describing developments over time in various fields of scientific inquiry.'<sup>133</sup> This statement highlights the traditional tendency for analytical work to be focussed on scientific knowledge, and its generation, rather than on the activity of science and its participants. That common approach has also promoted a kind of end-directedness. The emphasis in many of the well-known philosophical models such as Kuhn's

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<sup>132</sup> Machamer, P., Pera, M. & Baltas, A. (2000) p.vii

<sup>133</sup> Machamer, P., Pera, M. & Baltas, A. (2000) p.1

‘structure of scientific revolutions’<sup>134</sup> and Lakatos’ ‘research programmes’<sup>135</sup> has been on theory and the means by which it is amended and replaced. Machamer, Pera and Baltas state that: ‘Of course, neither scientists nor philosophers have been unaware of controversies. Nevertheless, they have been reluctant to recognise when and how controversy plays a constitutive role in the development of scientific knowledge.’<sup>136</sup> The focus on scientific knowledge has in some cases been divorced from attention to the activity that generates knowledge.

Placing emphasis on scientific knowledge over scientific activity forces attention on the achievement of closure and its outcomes. A knowledge product is only generated at the point of closure, when consensus forms regarding the validity of a knowledge claim. Knowledge is not produced during protracted negotiations. Models of knowledge production thus have no particular interest in the structure and dynamics of individual episodes of anomaly negotiation in science. Rather, they concern themselves with the influence of the knowledge outcomes of conflicts on the overall theoretical body of ‘science’. Since the generation of knowledge appears to occur with the termination of conflict it is that event that attracts attention. So, although we might think of these models as addressing conflict in science, they do not in fact deal with the details of negotiation, only its outcomes and how those relate to the body of knowledge that constitutes science. Since, as Englehardt and Caplan point out, attention to ‘change and development’ of scientific theories has dominated analytical work, it is unsurprising that a legacy remains which neglects the internal dynamics of conflict negotiation in favour of a focus on the achievement of closure and the generation of knowledge claims that closure implies.

The rise of sociological discourse concerning the internal process of scientific ‘change and development’ has to a degree addressed this problem of end-directedness; further attention has been turned to the processes of anomaly identification and negotiation. Sociologists of science have made significant attempts to counter the preconceived notions of science. They have made explicit the

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<sup>134</sup> Kuhn, T. (1962)

<sup>135</sup> Lakatos, I. (1976); Lakatos, I. (1970)

<sup>136</sup> Machamer, P., Pera, M. & Baltas, A. (2000) p.3

relation of 'contingent' activities to the production of constitutive scientific knowledge claims.<sup>137</sup> The concept of boundary work in particular comes closest to escaping common problems.<sup>138</sup> It avoids essentialism, and it accounts for the role of individual and group motivations/interests in the structure and dynamics of debate. Importantly, it also potentially allows for a discussion of conflict negotiation that need not be directed towards the achievement of closure. Boundary work can theoretically be conceived as a continuous and necessarily ongoing process. It can be understood to have no end, since boundaries must be constantly defended, redefined, extended and challenged. However, closure remains paramount in many of the treatments in which the constructivist models are tested against case studies; perhaps a legacy of the dominant 'scientific knowledge' studies that pre-date the sociological treatments. Where a constructivist approach, for example boundary theory, is used to explore a conflict case study it is chiefly applied to explicate a completed act or controversial episode. Overall, as sociologist Bart Simon has pointed out: '...few constructivist studies of controversy have focussed on the origins of controversies and the formation of core-sets'.<sup>139</sup>

Theoretical tools have been formulated in the context of the inherent belief that science should be, and invariably will be, defined by the resolution of conflicts. The result is descriptive tools that betray that optimism and reinforce those goals. As a result models/treatments often focus strongly, if not entirely, on the means by which the resolution is achieved. The rise of an anomaly at the foundation of a conflict is assumed. The structure, dynamics and duration of negotiation are comparatively irrelevant. And the conclusion and closure of the conflict is identified as the point of analytical interest and as the defining moment of the scientific enterprise.

In the context of an anatomy of scientific conflict that hinges upon the three-part process - anomaly identification, negotiation and resolution - protracted or apparently irresolvable controversies seem to defy explanation. Indeed, they might

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<sup>137</sup> See for key arguments on this issue: Collins, H. M & Pinch, T. J. (1979)

<sup>138</sup> For an outline of boundary work theory see Gieryn, T. F. (1995)

<sup>139</sup> Simon, B. (2002) *Undead science: science studies and the afterlife of cold fusion*. Rutgers University Press, New Jersey. p.24-25

simply seem uninteresting in as much as they lack a closure moment and all that that signifies. For sociologists the protraction of a controversy is not necessarily problematic; it does not interfere with theoretical assumptions. However, from the point of view of philosophy, protracted controversies apparently reveal a breakdown in the imagined process of the acquisition of scientific knowledge; the terms by which essentialists demarcate science largely rely on the power of the scientific methodology to resolve conflict through its 'special' nature. Protracted controversies imply that these demarcation criteria might not withstand application to real life episodes in science or explain the dynamics of the production of scientific knowledge. From a popular point of view, protracted conflicts threaten to undermine the special nature of scientific activity and the objective knowledge claims that it is believed to generate. Protracted controversies appear as aberrations, or at best perhaps as 'unfinished' conflicts that given time will eventually fall within the descriptive power of end-directed modelling tools. From a historical perspective protracted controversies represent unfinished tales that can tell us little until they have reached their conclusion, and which have no real entertainment value until it is certain how events will conclude. It is my intention in this project to shift the analytical gaze to focus particularly on the neglected intricacies of negotiation.

### **1.7 Perpetuating forces: an alternative to the passivity of end-directed controversy analyses**

End-directedness has resulted in limited theoretical attention to the specific character or dynamics of protracted phases of negotiation or conflict. Although the concept of boundary work goes some way to addressing these issues, it does not explicitly label boundary activity as an active force for the *perpetuation* of negotiation. It explains some of the means and motivations of negotiation, but does not extend its descriptive power to an explanation of how boundary work relates to the overall dynamics of controversy episodes. It does not provide analysis of how boundary activity relates to controversy protraction.

Examination of the structure and dynamics of protracted controversies, such as the directed mutation debate, highlights the central problem associated with this perception of and approach to controversy. The forces of perpetuation are resigned, in the majority of models, to exist in what we might think of as an interruption phase; protracted debates are considered suspended in the negotiation phase. Models generally predict that an anomaly will arise, that it will be perceived by the appropriate community, that a phase of negotiation will ensue, and that finally, the anomaly will be resolved by any number of processes such as assimilation, abandonment or redefinition.<sup>140</sup> In a protracted controversy, it is essentially imagined that this process is stuck in the negotiation phase – that there has been an interruption at that point in the process through which scientific activity should proceed. That evaluation renders protracted negotiation a passive phenomenon, with resolution ultimately inevitable. This is a subtle distinction, but one that is important in terms of its tacit classification of the duration of negotiation as a passive variable, and the achievement of closure as the active principle of conflict.

To consider protracted phases of negotiation as a passive phenomenon, or as part of a process *en route* to inevitable closure, results in the unfortunate side effect of overlooking the very features that characterise and determine the protraction of long running conflicts. It is during that period of interruption and drawn out negotiation that various features specific to the conflict, and the community involved in it, can actively contribute to the perpetuation of the controversy and its failure to reach closure.<sup>141</sup> It is those active factors of perpetuation that I want to identify in this project. Boundary work tells us how these aspects of negotiation are executed,

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<sup>140</sup> For a detailed discussion of the various modes of closure see: Engelhardt, T. & Caplan, A. (Eds.)(1987)

<sup>141</sup> In Engelhardt, T. & Caplan, A. (Eds.)(1987) the contributing authors explore the influence of political and ethical content and connotation on the structure and dynamics of scientific disputes. They effectively identify such content as a perpetuating force of controversy negotiation, although they express that perpetuation of negotiation instead as impediment to closure; as such they render the perpetuation of negotiation passive and assert closure as the inevitable goal of the conflict activity. It is worth noting that distinction of expression, since it illustrates again the general tendency of theorists to focus on the end directed achievement of controversy closure rather than on the structure of the negotiation phase in its own right. In spite of this distinction it is clear from that text that political and ethical content would qualify as a perpetuating force of the kind being described here. That category is not significant to the case of the directed mutation debate, although it would contribute a category of perpetuation to the 'general anatomy' of protracted controversies to which this project might contribute.

but does not follow through to make statements about how the efforts of the participants relate to the dynamics of the conflict.

The ‘closure study’<sup>142</sup> (discussed above) does adopt a view of closure impediments as an active principle in those controversies that are difficult to resolve (ie. in controversies with moral or ethical repercussions). However, the project gives less attention to the extent to which these impediments might exist as general perpetuating forces, alongside other active forces, as part of a general anatomy of scientific controversy. Essentially, the authors have identified what might be seen as one of many such forces. On account of their focus on policy-making and legislation, they have overlooked the relation that their concept of moral and ethical impediments to closure might have to a general anatomy of scientific conflicts and the forces that prolong them.

This project seeks to turn the focus explicitly upon the protraction of the negotiation phase, not in relation to the achievement of closure (so the language of impediments to closure is avoided), and as an active rather than passive principle. Certain categories of these active forces are identified in the following chapters (3-5) and are described for the purposes of this project as ‘perpetuating forces’. This selective catalogue of potential perpetuating forces represents a contribution to a possible anatomy of protracted controversy that this project aims to offer.

It is worth noting the potential for ‘perpetuating forces’ to increase the symmetry of controversy studies. As mentioned above, David Bloor introduced the concept of the ‘symmetrical’ approach during the 1970s as part of the Strong Programme.<sup>143</sup> He stated that in order to be symmetrical as historians of science we must consider both ‘true’ and ‘false’ claims/beliefs in science to have the same kinds of explanation. We should not dignify or give special attention to the scientific theories/knowledge that we deem to be ‘correct’. We should not think of ideas in the history of science as having been right or wrong; such present-centred evaluation, according to Bloor, is unproductive. For example, if politics, interestedness, economics and partisanship

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<sup>142</sup> Engelhardt, T. & Caplan, A. (Eds.) (1987)

<sup>143</sup> Bloor, D. (1976)

are invoked as active factors in the production of false claims, then those causes must also be sought out in the production of true claims. Rationality alone should not be deemed the hallmark of successful scientific activity. Bloor's attention to symmetry was a reaction against the kinds of explanation that assume that 'true beliefs' have internalist, essentialist or rationalist explanations, whilst 'false beliefs' can be attributed to external or social factors. This asymmetrical approach is the result of a teleological view of science; the explanations it offers are engineered backwards from a known outcome. That approach bears the serious burdens of present-centredness and Whig history<sup>144</sup>.

There is a similar problem of asymmetry in studies of scientific controversy. The tendency towards end-directed analyses and closure studies means that we begin to dignify resolved controversies, in a similar way to how we might asymmetrically dignify 'true' knowledge claims. To focus on closure, and reverse engineer an explanation of the means by which that resolution has been achieved, is problematically teleological and present-centred. Models of controversy borne out of attention to closure are only tested against completed conflicts, and so they are only tried against the very circumstances that they were designed to describe. Even where a thorough sociological discussion of the negotiation phase is given, for example in many of the boundary work studies, that explanation still generally pertains to an outcome that was known in advance (i.e. the expulsion of a scientist, or the monopolization of a scientific field). The means by which the outcome has been achieved are being explained, rather than the circumstances of the debate in their own right. In this sense, completed controversies become 'successes' in the same way as the asymmetrical treatments of knowledge see 'true' beliefs as successes. And the models that test them appear successful because they fit the terms of the problem that they were created to test; a kind of self-fulfilling prophecy. Unresolved controversies are considered pending completion, rather than considered as having any special character that determines their protraction.

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<sup>144</sup> Herbert Butterfield first used the term 'Whig History' to describe the problematic representation of the past by historians as a narrative of teleological progress towards the present, often involving heroes, goals and transhistorical presentations. Embedded in Whig history is a dedication to present-centred evaluations of the past. See: Butterfield, H. (1931) *The Whig interpretation of history*. G. Bell, London.

The attempt in this project to study an unresolved controversy, and to focus attention on the anomaly identification and negotiation phases, aims to achieve a better degree of symmetry. I suggest that the concept of perpetuating forces might help promote that symmetry, since it encourages the exploration of causes that do not dignify rationality or internalism above partisanship or politics, but rather seeks the active elements of controversy protraction across a diverse range of scientific controversies. For example, looking at the influences of categories such as ‘advocacy’, ‘history’, and ‘interdisciplinarity’ does not imply a particular outcome, or rely on any outcome to make their study relevant.

### **1.8 Conclusion: Towards an anatomy of protracted controversy: the contribution of this project to the wider discourse**

Analyses of scientific controversy have come from philosophers, sociologists, historians, policy makers, legislators and popular science writers. Essentialists have attended to controversy in relation to the demarcation problem, and constructivists have followed by addressing conflict as part of the process of articulation of the cultural territory of science. Some treatments are focussed on the dynamics of knowledge during a conflict, while others describe the activities of conflict participants. Popular authors have co-opted scientific controversy for the purpose of entertainment, and the media has found it to be an abundant source of ‘stories’. There is no scarcity of attention to this complex aspect of scientific activity.

Whether popular or professional, these treatments have been formulated within the even more complex context of the existing perceptions of science and conflict. Controversy has been variously attributed as a side effect of science, an integral part of its process and an aberration symptomatic of ‘failed’ science. There has been a general tendency to focus attention on the resolution or closure of controversy, a phenomenon perhaps borne out of the kind of disdain for scientific conflict described in section 1.6. As a result, the anatomy of controversy, to which



these treatments have all contributed, is dominated by the image of controversy as a three-part process of anomaly identification, negotiation and resolution. Resolution appears as the ultimate goal of conflict activity.

This construction of controversy is problematic in as much as it draws attention away from the negotiation phase, wherein the actual material of the controversy is in fact located. Constructivist sociologists have attempted to redress that balance, but even their negotiation-focussed tools tend to be deployed for the analysis of completed episodes of conflict, in which the resolution (the expulsion of an individual from the scientific community for example) is the ultimate inspiration for the study. So, even in the case of the constructivist models a pitfall exists of the same nature that the problem of 'present-centredness' creates in the history of science.

In this project I argue that one means by which to focus less goal oriented attention on negotiation is to think about the protraction of negotiation in terms of the active forces that lead to its perpetuation; I refer to these as perpetuating forces. That shift of emphasis promotes attention to elements such as personality, partisanship, culture and motivation, all of which sociologists have been keen to prioritise. Focussing on forces that contribute to perpetuation also reduces the problems of present-centred or teleological analyses, i.e. those that start with a closed conflict and attempt to work backwards to establish its means of negotiation and closure. Attention to the active forces of perpetuation allows a more self-contained and symmetrical analysis of negotiation. Particular episodes that constitute that phase can be examined in their own right, as active phenomena without necessary appeal to the relation they bear to conflict resolution. The remainder of this project will be concerned with identifying and discussing a selection of those perpetuating forces, using the synthetic methodology that I have outlined here.

## **Chapter 2: Case study: the directed mutation debate. A protracted scientific controversy.**

The ‘directed mutation’ controversy arose in the late 1980s, precipitated by observations of bacterial mutation that conflicted with Neo-Darwinian evolutionary theory.<sup>145</sup> Neo-Darwinian theory states that genetic mutation is random with respect to the quality of the environment. In that scheme, organisms become adapted to their environment only when they chance to experience a rare beneficial random mutation. The organism has no control over its ability to achieve adaptation at the genetic level, and the conditions of the environment cannot direct the mutational process towards an adaptive outcome. Thus, the evolutionary process is ultimately a precarious one, borne out of accumulated ‘lucky’ adaptive events.

The early reports of directed mutation described an alternative adaptive mechanism in some bacterial species. Researchers claimed that bacteria subjected to environmental stress, in the form of withdrawal of their accustomed nutritional substrate, were able to undergo a process of ‘directed mutation’ that enabled them to use an alternative food source. Rather than having to rely on the occurrence of a rare beneficial mutation to resolve the environmental challenge, it seemed that the bacteria were able to undergo mutation of exactly the kind that would render them adapted to the new conditions. Their mutational strategy was being directed by the quality of the environment, and their adaptation was not just fortunate, but rather purposeful.

The notion of environment directed mutation conflicts with Neo-Darwinian theory in two important ways. Firstly, it implies that non-random mutation underlies the adaptive process, which is contrary to the fundamental notion of ‘random’ mutation in Neo-Darwinism. Secondly, the achievement of directed mutation would rely upon communication between the environment and the genome. To direct

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<sup>145</sup> The term Neo-Darwinism refers to the evolutionary theory constructed from a combination of Charles Darwin’s theory of natural selection [as described in Darwin, C. (1859) *On the origin of species by means of natural selection*. John Murray, London.] and the theory of Mendelian inheritance. Neo-Darwinism was synthesised during the 1930s and 1940s by the architects of the ‘modern synthesis’, for example Theodosius Dobzhansky, Ernst Mayr and Julian Huxley. Neo-Darwinism is synonymous with the modern synthesis. More recently other elements have been combined with the theory, for example the new molecular genetics, manifest in concepts such as Francis Crick’s ‘central dogma’ (see section 3.2.3). Neo-Darwinian theory has been the foundation of the orthodox study of biological evolution since the mid-twentieth century. (Ridley, M. (1996) *Evolution*. Blackwell Science, Cambridge, Massachusetts.)

adaptation the genome would have to somehow perceive the quality of the environment and react to its demands. Neo-Darwinian theory asserts that the genome is a closed system, and that information travels only uni-directionally from DNA to RNA to protein.<sup>146</sup> In the Neo-Darwinian system there is no route of communication between the environment and the DNA

In essence directed mutation is non-Darwinian. In addition, it also implies a further level of dissent. The notion of cell perception, or even choice, that underlies the capacity to direct the mutational effort invokes the already highly controversial Lamarckian evolutionary theory.<sup>147</sup> Lamarckian theory supposes that organisms 'strive' for their adaptation and acquire traits during their lifetime that can be passed to offspring to promote their success. Classical Lamarckian theory has been forcefully rejected by the orthodox scientific community, as have the numerous incarnations in which its resurrection has been attempted. So, directed mutation was not only non-Darwinian dissent, but was also part of a historical legacy of Lamarckian resurrections.

This chapter provides a narrative reconstruction of the events of that debate. It seeks to recover the details of the two sub-debates that comprised this controversy; first, the molecular genetics debate concerning bacterial mutational processes and second, the broader conflict precipitated by the Neo-Darwinian response to an apparently Lamarckian phenomenon. This chapter is largely non-analytical (see Section 2.5), aiming chiefly to provide an illustrative resource to be drawn upon in subsequent chapters in which more general analysis of scientific controversy is undertaken.<sup>148</sup>

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<sup>146</sup> The notion of strictly uni-directional transfer of information from DNA → RNA → Protein has been a tenet of Darwinian theory since the 1970s. It is termed the 'central dogma' and was established by Francis Crick. [Crick, F. (1970) The central dogma of molecular biology. *Nature*, 227: 561-563.] Directed mutation implies an exception to this supposedly overarching description of genetic behaviour. The central dogma is described in more detail in section 3.2.3.

<sup>147</sup> Lamarckian evolutionary theory was first outlined in French naturalist Jean-Baptiste Lamarck's *Philosophie Zoologique* in 1809. [Lamarck, J. B. (1809) *Philosophie Zoologique*. (trans. H. Elliot, 1984) Chicago University Press, Chicago] Lamarck promoted a theory of species transformism at a time when the majority of people were dedicated to the fixity of species. Lamarck imagined that species could change over time and that they achieved this by means of an 'internal drive' allowing them to 'strive' towards a form that would better suit the conditions of their environment. The history of Lamarckism and the question of the theory's current status are very complex issues. These are discussed further in Chapter 3.2

<sup>148</sup> Appendix 2 features a 'cast of characters' glossary to support reading of this case study.

However, this chapter does rely on some methodological tools to facilitate the narrative reconstruction.<sup>149</sup> The description of the advocate and adversary groups engaged in this conflict is enabled by sociologist Harry Collins' core-set terminology.<sup>150</sup> Although participants in a debate may not be cohered by visible bonds such as shared institution or epistemology, Collins' core-set encompasses all those individuals allowing that they be perceived as a kind of self-contained community. He states that: 'This set of persons does not necessarily act like a 'group'. They are bound only by their close, if differing, interests in the controversy's outcome.'<sup>151</sup> This is an ideal descriptive tool for the international community of directed mutation participants, whose disciplinary and institutional affiliations are diverse, but who have an enduring interaction as participants in the debate.

The terminology of boundary work theory and the cartographic metaphor is also employed.<sup>152</sup> That language allows that the directed mutation debate be framed as a boundary dispute pertaining to the relative authority of the domains 'molecular biology' and 'evolutionary biology'. In that construction, John Cairns' particular brand of advocacy is illuminated as boundary work for expansion, while the defence of Darwinism by the critics of directed mutation is boundary work for protection of monopoly.

## **2.1 The directed mutation debate is comprised of two sub-debates**

The directed mutation debate has been protracted, enduring in various forms for two decades without resolution. The controversy is comprised of two sub-debates, which I refer to as the two 'aspects' of the debate. One aspect relates to the specific dissent concerning the legitimacy of the bacterial mutation observations. The other relates to the broader contest that the anomaly provoked, concerning the implications of directed mutation for evolutionary biology more generally. These two aspects are intertwined, and have each been periodically the more or less dominant strand in the overall debate. The emergence, nature and relation of these two aspects are outlined in this section.

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<sup>149</sup> These methodological tools are discussed in more detail in Chapter 1.

<sup>150</sup> See Collins, H. (1985); Collins, H. (1981b) The place of the 'core-set' in modern science: social contingency with methodological propriety in science. *History of science*, 19: 6-19.

<sup>151</sup> Collins, H. (1985) p.142

<sup>152</sup> See Gieryn, T. F. (1995). Boundary theory is described in Chapter 1.

To describe the history of this debate, and the emergence of its two aspects, it is necessary to select some logical point from which to commence a narrative. Following precedent<sup>153</sup>, I have selected the publication of the journal article 'The Origin of Mutants'<sup>154</sup> to serve that purpose.<sup>155</sup> The research reported in that paper was conducted by a team of molecular biologists at The Harvard School of Public Health, led by esteemed biologist Professor John Cairns. This paper is significant for several reasons:

i) 'The Origin of Mutants' includes the first report of 'directed mutation', and begins the aspect of the debate in which the legitimacy of this molecular biological anomaly is negotiated. The many other papers that combine to comprise that negotiation proceed from this paper, often citing it as the foundation of the conflict. The paper was the first to report that, in bacteria subjected to nutritional 'stress', adaptations seemed to arise as a specific response to that immediate environmental challenge. The Harvard team reported that non-lactose digesting *Escherichia coli* (Lac-) bacterial populations, when raised in the absence of their accustomed food, but in the presence of lactose, reverted at high frequency to the Lac+ form which is able to utilise lactose as a nutritional substrate. Only those bacteria that made the Lac reversion were able to survive the enforced starvation by adapting to use the unaccustomed lactose nutrition source that had been provided. It seemed that this adaptive Lac+ reversion was occurring at a frequency in excess of

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<sup>153</sup> In her 1993 review, Patricia Foster, an advocate of Cairnsian directed mutation, identifies Cairns, Overbaugh and Miller (1988) as the trigger for the debate. [Foster, P. (1993)] Also, popular accounts commonly identify that paper as the primary reference on directed mutation. [see Corliss, W. (1994) Adaptive mutation. At: [www.science-frontiers.com](http://www.science-frontiers.com) and Latter, J. (1999) Directed mutation and Galileo. At: [www.members.aol.com/joralat/direct.html](http://www.members.aol.com/joralat/direct.html)] In addition, John Cairns describes that publication as the 'paper that set off all the brouhaha' (personal communication 10/03).

<sup>154</sup> Cairns, J., Overbaugh, J. & Miller, S. (1988)

<sup>155</sup> This paper was not the first to address the function of mutation in starving bacteria. Research published earlier in the 1980s [Hall, B. (1982b) Evolution on a petri dish: using the evolved B-galactosidase system as a model for studying acquisitive evolution in the laboratory. *Evolutionary Biology*, 15: 85-149.] had described anomalies of bacterial mutation under starvation conditions. However, these earlier reports were published without any apparent boundary work activity, and significantly without reference to Lamarckism (contra Cairns, Overbaugh & Miller, 1988). The publications only described apparent directed mutation in bacteria as a genera-specific anomaly, with no reference to the broader implications of the phenomenon. That earlier material was not part of a challenge to the authority of evolutionary geneticists.

what could be achieved by the random mutation that Darwinian theory relies upon.<sup>156</sup> Rather than random mutation underlying bacterial adaptation/evolution, it seemed that an alternative adaptive mechanism was operating to permit the bacteria's acquisition of specific genetic traits aimed at the resolution of the nutritional stress. This aspect of the debate focussed on the methodological and interpretative conflict between those who presented and supported directed mutation data, and those who considered the experiments poorly conducted or badly interpreted.

ii) The paper also introduced the more general aspect of the dissent, invoking the ongoing conflict between Neo-Darwinian theorists and Lamarckian resurrectionists. That aspect concerned the implications of directed mutation for evolutionary theory. The control experiments reported in the paper showed that the presence of lactose in the medium was an essential pre-requisite for the occurrence of the Lac<sup>+</sup> reversion at this high frequency. That is to say, under other conditions of nutritional stress, i.e. the substitution of an alternative food substrate from lactose, the lactose revertants did not accumulate at the same rate as in the lactose plated cultures.<sup>157</sup> This implies that while adaptive mutations are accumulated in these populations, neutral mutations are not concurrently accumulated. Therefore, the mutation events occurring were not only adaptive, but were also *specific* to the conditions of the environment. It seemed that the bacteria could engage with their environment and respond in a manner appropriate to its demands. This combination of 'adaptivity' and 'specificity' pointed to environment directed modification of the genome. Thus, directed mutation is not only distinctly non-Darwinian, but is also essentially Lamarckian. 'The Origin of Mutants' instigated what would later be received as an attempt to resurrect Lamarckian theory.

iii) 'The Origin of Mutants' is also significant in that it reveals a boundary/authority dispute. It presents directed mutation as a concern not only for

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<sup>156</sup> That rate of reversion was far in excess of the estimated rate of spontaneous mutation known in *E. coli*. The spontaneous mutation rate for *E. coli* had been established in: Drake, J. (1969) Comparative rates of spontaneous mutation. *Nature*, 221: 1132.

<sup>157</sup> Cairns, Overbaugh and Miller (1988), controlled their lactose experiments with observations of valine resistance (Valr) mutation. They showed that a population of cells accumulating lactose digesting ability in the lactose medium (adaptive mutation) were not concurrently accumulating valine resistance (Valr) (a neutral mutation in this medium).

molecular biologists, but also for evolutionary biologists. It asserts the debate as an interdisciplinary conflict. As such, the paper had a significant role in shaping the nature and quality of the debate that followed. The authors use rhetoric in this paper to shape their observations as an appeal for an extension of the authority of molecular biology into the territory of evolutionary biology: that is, an appeal to be able to describe phenomena that are normally restricted to the domain of evolutionary biology.

In 'The Origin of Mutants' the Harvard team go to lengths to emphasise the potential gravity of the directed mutation anomaly. They did not shy from the implications of their observations for other types of cells than bacteria, nor did they avoid the pressing connection of their observations with Lamarckism. Indeed this paper embraces Lamarckian rhetoric. The abstract includes the assertion that: '...cells may have mechanisms for choosing which mutations will occur'.<sup>158</sup> This phrase is the opening gambit in a rhetorical battle that underpins the Lamarckism versus Darwinism aspect of the debate. It also reflects Cairns' approach to advocacy (discussed in section 4.2). Firstly, the use of the word 'choose' invokes classical Lamarckian theory, in which organisms are considered to achieve adaptation consciously through certain deliberate actions. Lamarck thought of organisms as 'striving' for adaptation and being participants in their evolution. The use of this term in the opening of the 1988 paper signalled Lamarckian association. Secondly, this phrase refers to a system in 'cells' rather than only in bacteria. This is a significant extrapolation from their observations in bacterial colonies to a much further reaching, more contentious statement concerning the possible existence of Lamarckian phenomena, not only in unicells, but also in the individual cells of multi-cellular organisms. The motives for invoking Lamarckism in this initial paper are considered in Chapter 4.2. It will suffice at this stage to recognise that in this initial publication Cairns' team made no attempt at apology or temperance in the presentation of their results or indeed in their evaluation of the implications of those results. The style of presentation in this paper forced a broad community to respond to the molecular biologists' claims.

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<sup>158</sup> Cairns, J., Overbaugh, S. & Miller, S. (1988) p.142

iv) Publication of 'The Origin of Mutants' corresponds to the inception of the 'advocates' section of the debating community or core-set. Points i) to iii) show the advocates fulfilling their first obligations as dissenters. First, they make a contentious unorthodox claim, and second, they adopt a rhetorical style that frames and coheres their dissent. In that paper they also assert their challenge by carrying out several other acts of boundary work.

For example, even at this initial stage Cairns' Harvard team addressed one of the major burdens facing the proponents of a contentious knowledge claim: to propose a causative mechanism for the observations.<sup>159</sup> Their proposal of mechanistic details in this paper indicates its significance as a foundation of a forthcoming debate. In 1988, Cairns' team favoured a mechanism involving a reverse transcriptase pathway.<sup>160</sup> They proposed the function of such a pathway in a strong and weak version. In the strong version, the cell was considered to have some organelle capable of assessing the variable mRNA sequences produced under conditions of nutritional stress for the usefulness of their protein product. This model stated that any mRNA with a beneficial end product could then be reverse transcribed back into DNA to allow the proliferation of its protein product. For example, in an environment where lactose was the only source of food a variable mRNA that coded for the enzyme lactase would have a very useful end product. That mRNA could be reverse transcribed back into DNA such that the genome would begin to produce lactase and starvation would be resolved. In the weaker version the same stress induced variable

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<sup>159</sup> The requirement that proponents of contentious scientific claims explain the causative mechanisms underlying their observations is identified and discussed in: Collins, H. M. & Pinch, T. J. (1979) and Darden, L. & Cook, M. (1994) Reasoning strategies in molecular biology: abstractions, scans and anomalies. In: D. Hull, R. Burian & M. Forbes (Eds.) *Philosophy of science association*. Vol.II: 179-191. Bruno Latour also references the requirement that causative evidence support contentious claims, noting that: 'when controversies flare up the literature becomes technical'. See Latour, B. (1987) *Science in action*. Harvard University Press, Cambridge, Massachusetts. p.30

<sup>160</sup> Reverse transcriptase is a polymerase enzyme discovered simultaneously in the 1970s by Howard Temin [Temin, H. & Mizutani, S. (1970) RNA-dependent DNA polymerase in virions of Rous sarcoma virus. *Nature*, 226(252): 1211-1213.] and David Baltimore [Baltimore, D. (1970) RNA dependent DNA polymerase in virions of RNA tumour virus. *Nature*, 226(252): 1209-1211]. This enzyme allows single stranded RNA sequences to produce double stranded DNA sequences by synthesis of a complementary nucleotide strand. This enzyme permits the replication of RNA viruses within their host, and was discovered through research to discover the mechanism underlying retrovirus reproduction. The discovery of this enzyme came as a major blow to the 'central dogma' [see footnote 19], rendering one half of the process of protein production from DNA bi-directional, rather than unidirectional as had been asserted previously.



mRNAs are the basis, but the reverse transcription of these is at random.<sup>161</sup> There are several factors that make the proposal of a reverse transcriptase dependent system tactically significant.

Firstly, the reliance of the system on the enzyme reverse transcriptase is rhetorically significant. Its simultaneous discovery by Howard Temin<sup>162</sup> and David Baltimore<sup>163</sup> in 1970 was a profound challenge to the Neo-Darwinian ‘central dogma’ of molecular genetics.<sup>164</sup> Invoking a link between the directed mutation observations and this molecule promised the benefit of association with previous work in molecular biology, that had shown that accepted accounts of molecular level evolution were incomplete and imperfect. The association of their results with the discovery of reverse transcriptase provided a theoretical context for their research; avoiding the stigma of isolation by association with previous efforts to re-evaluate the premise of the ‘central dogma’.

Secondly, a decade before this publication, Lamarckian resurrectionist Edward Steele had invoked reverse transcriptase in his proposed mechanism for the accumulation of acquired characters in the immune system.<sup>165</sup> This further enriched the context for the directed mutation observations, since by this time Steele’s work had enjoyed a good degree of acceptance in mainstream immunology. Association with this work implied the possibility that Steele’s observations could be only the tip of the iceberg, indicating a much more far reaching role for reverse transcriptase in

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<sup>161</sup> Cairns, J., Overbaugh, J. & Miller, S. (1988)

<sup>162</sup> Temin, H. & Mizutani, S. (1970)

<sup>163</sup> Baltimore, D. (1970)

<sup>164</sup> The ‘Central Dogma’ refers to the perceived unidirectional genetic pathway from DNA – RNA – Protein that had become a fundamental tenet of Darwinian molecular genetics by the late twentieth century [Maynard-Smith, 1993b *The theory of evolution*. (3<sup>rd</sup> Ed.)Cambridge University Press, Cambridge] The phrase was coined by Francis Crick to describe the universality of unidirectional information transfer from the genome to the phenotype. [ See: Crick, F. (1970)] This dogma precludes the direct modification of the genome by environmental factors, and renders the genetic material inviolate from change initiated at the somatic level. [See: Crick, F. (1958b) On protein synthesis. *Symposium of the Society for Experimental Biology*, XII: 153. Academic Press, New York.] For further discussion of Crick’s dogma see Chapter 3.

<sup>165</sup> Steele had shown that offspring could inherit immunity that their parents had acquired during their own lifetimes, and that this occurred by a feedback mechanism mediated by reverse transcriptase. The mechanism added the genetic material of parental immunity to the heritable genome. [See: Steele, E. (1979) *Somatic selection and adaptive evolution: on the inheritance of acquired characteristics*. Williams & Wallace International Inc, Toronto.] This immune process was accepted as part of established immunity theory by the time that Cairns published his early work on directed mutation in bacterial populations. See: Steele, E. (1981) Lamarck and immunity: a conflict resolved. *New Scientist*, 89: 360-361.

molecular genetics than was perceived at the time of its description in the 1970s. Steele himself had made use of Lamarckian references in his 1970s immunology work and went on to make even more radical pro-Lamarckian comment in his book *Lamarck's Signature*.<sup>166</sup> Thus, association with Steele's work also served to invoke an ideological context for the bacterial work of the Harvard team.

Together, the content and style of 'The origin of mutants' precipitated and shaped the debate that followed. Its combination of anomaly reporting, causative theorizing, rhetoric and association making, marked it as the foundation of an authority challenge to evolutionary biologists by molecular biologists. The style of the paper demonstrates the authors' careful attention to the obligations of 'dissenters' and a self-aware approach to their unorthodoxy. The provision of possible causative explanations of directed mutation and the use of Lamarckian references can be viewed as acts of boundary work for the forthcoming authority struggle. Cairns' team had not only reported an anomalous finding, but they had framed the anomaly in such a way as to construct an effective boundary challenge.

The outcome of this presentation was the emergence of the two-aspect debate. First, there was a contested knowledge claim concerning the existence of a directed mutation phenomenon in bacteria, or possibly in 'cells'. Second, there was the implied significance of the phenomenon for evolutionary theory. From this double challenge emerged the specific and defined molecular genetics debate, alongside the broader debate on evolutionary theory. In relation to the technical molecular biological debate Cairns' team had introduced their observations to an appropriate forum for debate. In relation to the broader debate, Cairns' had essentially 'thrown down the gauntlet'. The style and content of the paper ensured that response to it would be forthcoming in relation to both aspects of the contest.

## **2.2 After 'The origin of mutants': the development of a controversy core- set.**

In this section, I describe the journal publications on directed mutation that appeared in the period 1988 to 1997. That period corresponds to the principally

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<sup>166</sup> Steele, E., Lindley, R. & Blanden, R. (1998) *Lamarck's signature: how retrogenes are changing Darwin's natural selection paradigm*. Allen & Unwin, St. Leonard's, Australia.

journal-based phase of the controversy. After 1997 the Internet forum assumes increased importance and becomes the principal forum for this debate in terms of scale of participation. That shift of forums is discussed in section 2.3 and analysed in detail in Chapter 5.

The published response to 'The Origin of Mutants' was immediate and prolific, ranging from specific criticisms of the paper, to further reports of the directed mutation phenomenon from other investigators. Understanding the response to that paper is key to understanding the structure and quality of the debate over the next two decades. To organise these publications I utilise Collins' 'core-set' terminology (although not without certain caveats which are discussed throughout this section).<sup>167</sup> The core-set is useful to this discussion in that it provides a means by which to represent those involved in the directed mutation debate as a type of community, even without any geographic or institutional cohesion. Rather, the principle of membership of the group exists in the fact of involvement with the controversy. Also, this membership need not be stable in terms of scale or composition, and is therefore suited as a description of the dynamic community that contributed to the directed mutation debate during its journal-based phase.

The core-set emerged directly after the publication of 'The Origin of Mutants'.<sup>168</sup> Its scale is difficult to determine, since it was characterised by fluctuation of membership, and even changing strategies on the part of its key members.<sup>169</sup> However, this core-set can be considered loosely composed of: i) protagonists and antagonists of the directed mutation phenomenon in molecular biology, and ii) protagonists and antagonists of the broader implications of directed mutation for evolutionary theory. This division is useful only with the caveat that

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<sup>167</sup> Collins defines and describes the core-set in: Collins, H. (1981b) and Collins, H. (1985)

<sup>168</sup> Collins acknowledges that it is often difficult to determine the point at which the core-set arises. He suggests that a good time to consider a core-set initiated is with the publication of some 'unambiguous claim' [Collins, H. (1981b) 12]. On that premise, I suggest that Cairns' 1988 paper represents emergence of the core-set in this case. Although other publications had described bacterial stress experiments, they had not been presented with the rhetoric of controversy, and their claims had remained ambiguous in terms of broad significance to evolutionary theory.

<sup>169</sup> Collins (1981b) acknowledges that core-set membership is difficult to determine precisely, since it is characterized by fluctuation of often-temporary members. In this case, I have selected the authorship of 3 or more journal articles pertaining to the controversy, or the undertaking of private correspondence with one of the principle authors, as signifying membership. Although this is not ideal, since it does not give a direct measure of an individual's impact on the controversy, it does at least reveal the key participants.

support for directed mutation as a principle of molecular biology does not automatically indicate support of directed mutation as the foundation of broader non-Darwinian or Lamarckian principles. In addition, the core-set has a significant component whose support of or antagonism towards the directed mutation research remains discrete by the nature of their publication forum (for example review articles or editorials where bias is discouraged). Numerous key publications, especially in the early period, come from reviewers whose articles are supposed to be at least ostensibly unbiased.<sup>170</sup>

After the mid-1990s, the core-set grouping loses some of its usefulness to this analysis, as the scale of the debate is changed dramatically by uptake in the Internet forum. The issues of sociological grouping for this debate are dealt with below in full. It will suffice at this point to appreciate, with the aid of the core -set terminology, that after Cairns' 1988 publication a debating group emerged to discuss directed mutation as both a bacterial phenomenon and a potential example of New Lamarckian evolution.

Initially, the antagonists' key objection was founded upon observations made in 1943 by Salvador Luria and Max Delbruck<sup>171</sup> during experiments on bacterial populations subjected to bacteriophage infection.<sup>172</sup> The experiments aimed to assess whether the phage resistance mutation arose at random in these populations with respect to time of infection, or was more likely to occur after the selective force of infection was applied. Essentially this would show whether the resistance mutation arose as a response to the phage infection (i.e. was directed by it) or simply occurred as a random mutation with no regard for utility. Luria and Delbruck developed an assay called the 'fluctuation test' that showed that phage resistance mutations arose as random events during the culture time of the bacteria, rather than more frequently after infection of the population by the phage. This random occurrence of beneficial mutations was demonstrated by the large variations in numbers of resistant bacteria between independently plated populations. For example, a culture in which the

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<sup>170</sup> See for example: Stahl, F. (1988) A unicorn in the garden. *Nature*, 335:112-113.

<sup>171</sup> Luria, S. & Delbruck, M. (1943) Mutations of bacteria from virus sensitivity to virus resistance. *Genetics*, 28: 491-511.

<sup>172</sup> Bacteriophage is a virus that infects and destroys bacterial cells.

resistance mutation had occurred early in the culture time would have a high number of resistance individuals by the time the phage was introduced. Therefore, once plated with the phage, this colony would appear to have a high number of resistant individuals. Conversely, a population which did not have any individual experiencing the chance resistance mutation during the culture time, or which only experienced this mutation late on, would not be so populated by the offspring of the resistant bacteria. Therefore, once plated with the phage the colony resistance would be much less significant than in the early mutant culture. Luria and Delbruck concluded that this variation was the result of the resistance mutation arising at random times during culture. Their fluctuation test results showed that the mutation to confer phage resistance was as likely to occur prior to infection with the phage as after. This showed that the selective agent (the bacteriophage) was not responsible for triggering mutations to confer resistance.

At first glance these results seem to preclude Cairns' observations; it seems that adaptive mutations do not arise more frequently after a selective pressure is applied. However, as Cairns' team were eager to emphasise, their assay conditions were very different from those in the Luria and Delbruck test; in fact, so different that a comparison between the two experiments is meaningless. The introduction to 'The Origin of Mutants' describes the Luria and Delbruck methodology, and explains the distinction between their study and that of Cairns' team. That discussion acted as a pre-emptive strike against the invocation of Luria and Delbruck as a precedent against their findings.

By studying starvation Cairns' team were studying the bacterial colony during 'stationary phase'. This is a non-growth phase entered by stressed bacterial cells.<sup>173</sup> In this phase genetic mutations can occur, and any mutation which allows the cells to survive the environmental conditions will allow that cell to re-enter growth phase and produce a sub-population of adapted cells. Luria and Delbruck's choice of the bacteriophage as the selective agent resulted in the death of the colonies immediately after infection if the resistance trait was not present having arisen in earlier culture. Their experiments therefore did not allow a stationary phase period in which mutation could occur *as a response* to the environmental impetus to adapt. They were not

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<sup>173</sup> Cairns, J., Overbaugh, J. & Miller, S. (1988)

observing post-selection adaptation, because their experimental design did not include a post-selective period in which the cells were able to survive and mutate. They had not examined the phase in which Cairns' team claimed to have observed mutation directed by the environmental conditions. Luria and Delbruck's fluctuation results were not in fact precedent against the directed mutation work. As Cairns himself has said, 'if you want bacteria to evolve, it's not fair to kill them'.<sup>174</sup>

Despite this crucial difference between the bacterial states studied by Luria and Delbruck and the Harvard team, the fluctuation test was repeatedly invoked in criticism of the new observations.<sup>175</sup> Critics of directed mutation were cohered during 1988 to 1993 by a common invocation of Luria and Delbruck as precedent against the Harvard' observations.<sup>176</sup> However, some authors were more perceptive concerning the significance of the stationary phase distinction and acknowledged this from the outset.<sup>177</sup> These individuals instead focussed their objections upon those specific aspects of Cairns' research that they found inadequate in terms of method or control. There were two common objections: discrete cell death/reproduction, and the appraisal of control mutations as neutral.

Linda Partridge and Michael Morgan first drew attention to these methodological issues. Their objections were detailed in the correspondence pages of *Nature* two months after 'The Origin of Mutants' appeared in the journal.<sup>178</sup> They suggested that Cairns' calculations of population size during stationary phase could have been inaccurate due to unaccounted for death or growth occurring during the stationary phase. This might occur if natural cell death was equalled by slow division

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<sup>174</sup> Personal communication with John Cairns, 10/04

<sup>175</sup> See for examples: Stahl, F. (1988); Lenski, R., Slatkin, M. & Ayala, F. (1989) Mutation and selection in bacterial populations: alternatives to the hypothesis of directed mutation. *Proc. Natl. Acad. Sci. USA*, 86: 2775-2778; Stewart, F., Gordon, D. & Levin, B. (1990) Fluctuation analysis: the probability distribution of the number of mutants under different conditions. *Genetics*, 124: 175-185; Lenski, R. & Mittler, J. (1993a) The directed mutation controversy and Neo-Darwinism. *Science*, 259: 188-194; Lenski, R. & Mittler, J. (1993b) Directed mutation. *Science*, 260: 1222-1223.

<sup>176</sup> In this period, evolutionary geneticists Richard Lenski and John Mittler, were the key core-set antagonists of directed mutation (Lenski was Mittler's PhD supervisor). They published prolifically and asserted Luria and Delbruck as precedent against directed mutation. Alongside them, evolutionary geneticist Bruce Levin (who had been post doctoral supervisor to Lenski and would later also mentor Mittler), not only published material but also engaged with John Cairns in a 'heated' exchange of private letters [personal communication with John Cairns 10/03]

<sup>177</sup> For example: Prival, M. & Cebula, T. (1992) Sequence analysis of mutations arising during prolonged starvation of *Salmonella typhimurium*. *Genetics*, 132: 303-310. In which the authors acknowledge the important distinction between the use of lethal (i.e. bacteriophage) selective agents of the Luria and Delbruck type and the use of non-lethal agents (i.e. starvation) of the Harvard type.

<sup>178</sup> Partridge, L. & Morgan, M. (1988) Is bacterial evolution random or selective? *Nature*, 336: 22

of some other cells, as a result of an impure medium. The result of these discrete growth phenomena would be that the population size would not appear to alter and yet growth would be occurring. If this was the case, then any mutants arising could be of a normal replication dependent type and could therefore be random with respect to the environmental quality rather than reliant on a reverse transcription pathway.

This methodological criticism did not account for the observation that neutral mutations did not accumulate alongside apparently adaptive mutations. However, they also questioned the definition of a neutral mutation. Cairns had identified Valine resistance (Valr) as a neutral trait and established that this did not accumulate alongside the lactase mutation in the lactose medium, thus demonstrating the specificity of the observed mutations. Partridge and Morgan (and later Danchin<sup>179</sup>) suggested that Valr might not be fully neutral, as it might raise the probability of cell death when in a lactose medium.<sup>180</sup> If that were the case, then a higher rate of mutation to Lac+ than to Valr would be expected in a lactose medium anyway.

Multiple objections of this kind emerged in a dedicated discussion section in the December 1988 issue of *Nature*. In that collection of letters, Holliday and Rosenberger point out that: 'It is a guiding principle in science that a radical new interpretation (in this case one invoking the inheritance of acquired characteristics) should only be considered if simpler explanations based on existing knowledge are inadequate.'<sup>181</sup> The contributions in that section all appear in service to that agenda<sup>182</sup>; their commentary collectively encourages that an explanation for the anomaly be sought that does not rely on unorthodox speculation.

These commentaries provided a programme for further research, which was addressed by the advocates of directed mutation in subsequent papers that comprised

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<sup>179</sup> Danchin, A. (1988) Origin of mutants disputed. *Nature*, 336: 527. Danchin expressed concern that the use of Valine resistance as a control for the lactose reversion was inappropriate, since it involved a frameshift mutation that might behave very differently from lactose reversion under selective pressure.

<sup>180</sup> Valr might raise the chance of cell death in a lactose medium on account of its physiological cost to the organism. Under the stress conditions of this medium a Valr trait may be too 'expensive' in terms of physiological energy investment for an organism to maintain life in a medium in which it has no survival value. This criticism is valid in terms of its appreciation of the economics of nature and associated survival, however, there is no evidence that Valr does carry a cost of this kind in the case of *E. coli* in a lactose medium.

<sup>181</sup> Holliday, R. & Rosenberger, R. (1988) Origin of mutants disputed. *Nature*, 336: 526.

<sup>182</sup> Charlesworth, D., Charlesworth, B. & Bull, J. (1988) The origin of mutants disputed. *Nature*, 336: 525; Grafen, A. (1988) The origin of mutants disputed. *Nature*, 336: 525-526; Holliday, R. & Rosenberger, R. (1988); Van Valen, L. (1988) The origin of mutants disputed. *Nature*, 336: 526; Danchin, A. (1988); Tessman, I. (1988) The origin of mutants disputed. *Nature*, 336: 527.

a period of active methodological debate.<sup>183</sup> Cairns responded at once, with his own letter appearing at the end of the discussion section.<sup>184</sup> In this he addressed each of the authors objections, disregarding several of them, but flagging others (i.e. the neutrality of Valr) as areas for investigation. Although Cairns acknowledged that his letter was 'primarily a response to our critics' he also used the opportunity to reassert his conviction that 'it seems almost perverse to maintain, as a matter of principle, that such a mechanism [directed mutation] has never evolved.'<sup>185</sup>

Other initial responses were more supportive of the Harvard research. Some, while not enthusiastic, or in agreement with Cairns concerning interpretations or mechanism at least regarded the potential anomaly as interesting or important. For example, two months after the publication of the Harvard results, biologist Spencer Benson wrote to *Nature* that: 'Although the actual experimental data presented are sketchy and the details of the experimental procedures are lacking, one is left with the feeling that the phenomenon reported by Cairns' *et al* is real and warrants further investigation.'<sup>186</sup> He adds that: 'I believe we are seeing a form of directed mutagenesis in this selection, though we have no data on how this directed response might occur.'<sup>187</sup> Others offered more detailed commentary in favour of directed mutation.

In particular, two individuals spoke out early in tentative support of the new research; Franklin Stahl and Barry Hall. Franklin Stahl, a molecular biologist from the University of Oregon, published an article entitled 'A Unicorn in the Garden' in the same issue of *Nature* as Cairns' paper.<sup>188</sup> This article appeared in the 'news and

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<sup>183</sup> For example, directed mutation advocate Barry Hall references the 'origin of mutants disputed' discussion in two follow up papers. Hall, B (1990a) Spontaneous point mutations that occur more often when advantageous than when neutral. *Genetics*, 126: 5-16; Hall, B. (1991b) Adaptive evolution that requires multiple spontaneous mutations: mutations involving base substitutions. *Proc. Natl. Acad. Sci.*, 88: 5882-5886. In those highly technical papers Hall focuses on methodology and the provision of experimental detail.

<sup>184</sup> Cairns, J. (1988) Origin of mutants disputed. *Nature*, 336: 527-528.

<sup>185</sup> Cairns, J. (1988) p.528

<sup>186</sup> Benson, S. (1988) Is bacterial evolution random or selective? *Nature*, 336: 21-22. p.21

<sup>187</sup> Benson, S. (1988) p.22

<sup>188</sup> Stahl, F. (1988). This was the first of three review articles published by Stahl on directed mutation. His tentative support was indicated in the cryptic title, which he later explained alluded to James Randi's rhetorical question 'What would you do if I said I keep a unicorn in my back yard?', that he posed during his discussion of the water memory debate that had been raging earlier that year in the pages of *Nature*. [Stahl, F. (1992) Unicorns revisited. *Genetics*, 132: 865-867. p.866]. The answer to this question Stahl says is 'I would climb over to have a look!' [Stahl, F. (1992) p.866] Stahl was inviting the reader to look more closely at Cairns' work and not be blinded by assumptions.



reviews' section, and while maintaining the style of a review, was generally positive about the future of research into directed mutation.<sup>189</sup> The article re-emphasised the distinction between Cairns' work and Luria and Delbruck's. Stahl even theorised a possible mechanism for the accumulation of directed mutations in bacterial populations. He suggested that the normal genetic mismatch repair systems of the stressed bacterial genome may become error prone, if not non-functional, and that as a result the genome may accumulate a larger number of mutations than at other points in the cell cycle. This increase in rate would make it more likely that an individual cell might achieve a beneficial mutation. In proposing this mechanism Stahl is suggesting that the mutations observed by Cairns may be the result only of faster mutation, rather than mutation specifically directed by the environmental conditions. Stahl is effectively offering a weak model as an alternative to the strong model proposed by the Harvard team. Although such a mechanism would strip some of the Lamarckism from directed mutation, the situation would remain that the bacterial cells have in place a mechanism by which to modify the quantity if not the quality of mutations at the time of an environmental challenge; a previously unforeseen capacity in any cells. Stahl qualifies his proposal of this weaker model, acknowledging a forthcoming paper by Barry Hall that promised to report experiments showing how several beneficial mutations can arise simultaneously when selected by the medium, without concurrent accumulation of neutral mutations.<sup>190</sup>

Barry Hall's 1988 paper was perhaps the most significant supportive response to 'The Origin of Mutants' that year.<sup>191</sup> Hall had been working with nutrition stressed bacterial populations throughout the 1980s at the University of Rochester in New

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<sup>189</sup> The significance of this paper is described further in Chapter 4. It is an example of 'publication with qualification' of the kind that Collins and Pinch have described (Collins, H. & Pinch, T., 1979). It was included alongside the Harvard paper to act as an editorial qualifier, encouraging the readership to view the new work with caution, if not suspicion. Similar qualifying material had appeared in *Nature* alongside Benveniste's water memory publication earlier that year [see Maddox, J., (1988a) Editors note: When to believe the unbelievable. *Nature*, 333: 787] appearing alongside Davenas, E., Benveniste, J. *et al.*, (1988). Although Stahl seems enthusiastic, he is encouraging a 'wait and see' attitude. In that 1988 paper he refers to forthcoming confirmatory material on directed mutation that he has been made aware of saying: '...be warned, however, that more difficult challenges are just over the horizon.' (Stahl, 1988 p.113) A telling expression, that reveals the function of this paper as a qualifier of the unorthodox publication.

<sup>190</sup> That paper appeared as: Hall, B. (1988) Adaptive evolution that requires multiple spontaneous mutations. 1. mutations involving an insertion sequence. *Genetics*, 120: 887-897.

<sup>191</sup> Hall, B. (1988)

York. He was therefore an obvious candidate to take on the challenge of replicating Cairns' results and to address some of the methodological criticisms. As Stahl had promised, Hall's paper did extend Cairns' work to describe the accumulation of multipoint adaptive mutations. Hall showed that even in a medium where two or more factors were limiting bacterial growth the environmental challenges were met at a much faster rate than could be achieved by chance alone, perhaps even as much as a hundred million times faster. In addition, Hall suggested that only the specific combination of mutations required would arise, without concurrent accumulation of mutations of no benefit. Hall had extended Cairns' experiments to encompass a study of multipoint mutations too complex to arise in any significant number by chance alone. The probability of achieving the multipoint mutation by random mutation processes would be extremely low, and yet Hall showed that these multipoint mutations could occur at similar rates to the single point mutations that the Harvard team had reported.

Hall also modified Cairns' assays to test for the occurrence of different types of mutation. Cairns' team had only studied the action of point mutations, involving the replacement of one nucleotide by another, a relatively simple mutation process. Hall extended his study to test for the ability to accumulate mutations requiring insertion or deletion of a nucleotide base, such that a frameshift mutation occurs. These mutations involved more complicated errors of replication and were therefore less likely to arise by chance alone.

Hall also devoted part of his discussion to mechanisms for directed mutation, as Cairns, Overbaugh, Miller and Stahl had done before him. His theoretical suggestions differed significantly from those of Cairns' team; if Cairns' model was the strong version of directed mutation, Hall's was a weak version. Cairns emphasised specificity and environment direction of mutation, Hall suggested a less directed stress-induced mutational response underlay the apparent directed mutations. Cairns' original strong model and Hall's weaker model came to represent the two main approaches pursued by the supporters of directed mutation in the 1990s.

Allegiance to one or other of those interpretations divided the protagonists into two schools of thought in the period after the initial publications.<sup>192</sup>

Hall focussed on what he called 'hypermutation' as the principle underlying the directed mutation phenomena.<sup>193</sup> This theory suggested that the stressed bacterial genome might have the capacity to enter a phase of extremely elevated mutation rates as a contingency plan for times of extensive environmental challenge.<sup>194</sup> In the stronger version of his theory Hall proposed that specific regions of the genome, associated with the digestive capabilities of the bacteria, would be the foci of hypermutation in the starvation phase such that the chance of achieving beneficial mutations of phenotypic significance would be enhanced. The mass mutation effort would be targeted specifically enough to allow the mutation of specific genes associated with the utilisation of a particular substrate as a nutrient. In the weaker version, Hall proposed a phase of *genome-wide* hypermutation. Such widespread mutation would almost invariably be disastrous for an organism. However, occasionally, this last ditch effort to survive the conditions would result in the creation of an adapted mutant that could exit stationary phase and breed a clone of adapted cells.<sup>195</sup> In the case of this weakest model, the 'directed' element of directed mutation is essentially lost. The capacity to hypermutate would be particular to bacteria, interesting in its uniqueness, but not significant in terms of implication for broader molecular genetics or evolutionary theory. Hypermutation in this form would represent a quirk of bacterial evolution, manifesting itself as a contingency plan for use in the most extreme hostile environments.

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<sup>192</sup> For example, John Cairns and Patricia Foster support the strong version, while Franklin Stahl and Barry Hall promote the weak version.

<sup>193</sup> Hall, B. (1988)

<sup>194</sup> This can be considered an active form of the mechanism described in Stahl (1988). Where Stahl (1988) considers the rapid mutation process passive and mediated through a breakdown of genetic repair systems under stress, Hall (1988) suggests that hypermutation is a controlled and evolved physiological process purposed for the acquisition of a vast range of potentially useful mutations.

<sup>195</sup> This theory is reminiscent of Richard Goldschmidt's concept of the 'Hopeful Monster'.

[Goldschmidt, R. (1940) *The material basis of evolution*. Yale University Press, Connecticut]

Goldschmidt proposed that one off grand scale mutants might have existed creating the palaeontological gaps considered as 'missing links'. These individuals can arise in one generation and may have extreme phenotypic differences from their parents, that nevertheless, confer some benefit in a changed environment. Hall's weak model invokes a similar image of a fortunate mutant arising from generally disastrous population of non-adaptive large-scale mutants.

Hall's 1980s papers did not address the Lamarckian associations that Cairns' team had been eager to emphasise.<sup>196</sup> He instead referred only to the experimental details, and their immediate implications for the bacterial system. He stated that the experiments had indicated a bacterial mechanism for dealing with extreme starvation situations, and did not refer to a theory for 'cells' in general as Cairns' team had done. Hall's advocacy of mechanisms less antithetical to Neo-Darwinism demonstrates fundamental differences between his agenda and that of Cairns' team. Hall's objective was to explore the bacterial anomaly, but his motivation did not stretch to an agenda of explaining the results in terms outside those permitted in a Neo-Darwinian treatment. Whereas the Harvard team had constructed their results as a challenge to the authority of evolutionary biologists, and as part of an attempt to increase the authority of molecular biology, Hall had not interpreted the anomaly in the same way. Whereas Cairns (and Steele (1979) before him<sup>197</sup>) had been eager to frame their observations as Lamarckian, Hall was more moderate and flexible in his evaluation of the phenomenon underlying directed mutation. Whereas Cairns moved keenly between specific data and broader speculation, Hall maintained a focus on bacterial genetics. Hall expressed interest in the anomaly from within his own discipline, and did not pursue the implication that other disciplinary groups should become involved in the negotiation. He did not make apparent any broader agenda, and did not contribute actively to the contest for authority that Cairns' team had framed.

The different character of Cairns' and Hall's advocacy of directed mutation is examined in detail in Chapter 4.2. In that section, I contrast their approaches as 'loud' versus 'quite' advocacy, and their styles are compared with those of Pons, Fleischmann and Jones during the Cold Fusion debate. For the purposes of this discussion it suffices to note that by 1990 their qualitatively different approaches marked the extremes of the range of positions adopted by advocates of directed mutation. Other advocates in the 1990s took up positions between those two extremes

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<sup>196</sup> Hall, B. (1982a) Evolution of a regulated operon in the laboratory. *Genetics*, 101: 335-344; Hall, B. (1982b); Hall, B. (1988); Hall, B. (1989) Selection, adaptation, and bacterial operons. *Genome*, 31: 265-271.

<sup>197</sup> In the 1970s Edward Steele had proposed a system of acquired immunity in mammals, and had emphasised the degree to which such a system would support a resurrection of Lamarckian theory. See Steele, E. (1979) and Steele, E., Lindley, R. & Blanden, R. (1998)

as they expressed support of directed mutation – becoming either more ‘Cairnsian’, or more invested in hypermutation theory.

The remainder of the 1980s discourse on directed mutation was dedicated mainly to the replication and slight modification of Cairns’ observations, with Hall offering the most comprehensive treatments.<sup>198</sup> These repetitions were met by critic’s proposals of unproblematic interpretations of what appeared as ‘directed’ evolution.<sup>199</sup> The advocates’ further investigations aimed to fulfil the obligations for methodological stringency that critics demanded. Cairns answered methodological queries and complaints both in the correspondence pages of *Nature* and through personal communication with several researchers.<sup>200</sup> Replicability is a key requirement for any new observation, and not until this requirement had been met would it be acceptable to extend the research to assays of other unicellular organisms or to a theory of directed mutation in multicellular organisms.

Hall was a major force in the effort to replicate and confirm the Harvard results. He published six papers between 1988 and 1991 that detailed observations of starvation experiments in *E. coli*.<sup>201</sup> His experiments showed that Cairns’ observations could be repeated using the same basic assays. In addition, each article made a small contribution to the perpetuation of the debate by extending the assays, for example, to describe directed mutation for other substrate utilization, or adaptations involving other types of mutation i.e. frameshift and substitution. In 1990 Hall confirmed that: ‘Evidence for Cairnsian mutations has now been found in all cases where it has been sought’<sup>202</sup>, and in 1992 he published observations of directed

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<sup>198</sup> Hall, B. (1988) Hall, B. (1989)

<sup>199</sup> Lenski, R. (1989) Are some mutations directed? *Trends in Ecology and Evolution*, 4: 148-150.

<sup>200</sup> In particular, Cairns entered into debate with evolutionary biologist Bruce Levin. Through letters, the two scientists debated the relevance of fluctuation analysis to the Harvard assays [Personal communication with John Cairns 10/03.] Levin published on this issue in 1990 [Stewart, F., Gordon, D. & Levin, B. (1990)] Bruce Levin was unwilling to release the materials of that private debate for the purposes of this project, although John Cairns stated that he would release his copies with Levin’s permission. In 2000 Levin co-authored a general article addressing the issue of adaptive evolution in bacteria. In that article he referred to Hall and Lenski’s work, but not to Cairns’ or Fosters’. [Levin, B. & Bergstrom, C. (2000) Bacteria are different: observations, interpretations, speculations and opinions about the mechanisms of adaptive evolution in prokaryotes. *Proceedings of the National Academy of Sciences, USA*, 97: 6981-6985]

<sup>201</sup> Hall, B. (1988); Hall, B. (1989); Hall, B. (1990a); Hall, B. (1990b) Directed evolution of a bacterial operon. *Bioessays*, 12: 551-558; Hall, B. (1991a) Spectrum of mutations that occur under selective and non-selective conditions in *E.coli*. *Genetica*, 84: 73-76; Hall, B. (1991b)

<sup>202</sup> Hall, B. (1990a) p.15

mutation in the unicellular fungus Yeast.<sup>203,204</sup> This provided the first evidence of the phenomenon in non-bacterial cells, and thereby, the first evidence in favour of Cairns' claim that directed mutation might be a process in 'cells'. This report also began to answer Hall's own demand that: 'If Cairnsian mutations are of evolutionary importance, then they must occur in a variety of organisms...'<sup>205</sup>

Yet, in his many publications Hall remained disengaged from the Lamarckian dissent that Cairns had raised, illustrating the character of his advocacy. Despite being engaged in scientific dissent he maintained a moderate approach, avoiding speculation. Hall's articles focussed on the presentation of data and proposals of mechanism. They were highly technical, and less discursive than Cairn's offerings. His presentation lacked the more inflammatory material concerning the implications of the theory for evolutionary biologists. Perhaps as a result, review material and correspondence more frequently addressed Cairns' publication than Hall's, although Hall consistently received citation in technical contributions to the debate.

Hall also did not engage explicitly in the authority contest that the Harvard publication had implied between molecular biology and evolutionary biology. He did not highlight the broad challenge to evolutionary biology or Darwinism. As a result, generally, the criticisms levelled at Hall were based on methodological queries. Hall responded to these by ensuring that his next publication incorporated the solution to identified methodological problems. However, in criticisms of Cairns' work there was a greater opportunity to take issue with the broader agenda, and to criticise ongoing attempts to resurrect Lamarckian theories.

The different response to Cairns' and Hall's work can be traced back to 'The Origin of Mutants'. The Lamarckian associations and speculative theorising in that paper instigated dissent. Hall had even published on adaptive mutation in bacteria in the early 1980s without provoking any similar response.<sup>206</sup> Whereas Hall was able to modify his future methodology to answer criticism, Cairns could not hope to so easily placate his critics since the resolution of his point of dissent required more than

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<sup>203</sup> Hall, B. (1992) Selection induced mutations occur in yeast. *Proc. Natl. Acad. Sci. USA*, 89: 4300-4303.

<sup>204</sup> Directed mutation in Yeast was also reported in: Steele, D. & Jinks-Robertson, S. (1992) An examination of adaptive reversion in *Saccharomyces cerevisiae*. *Genetics*, 132: 9-21.

<sup>205</sup> Hall, B. (1990a) p.6

<sup>206</sup> Hall, B. (1982b)

appeal to data or modification of methodology for resolution. Critics' response to the two authors took on the quality that Cairns had predicted in 1988 when he stated that:

'At its extremes it was an argument between reductionists and romantics - between those who sought to explain the evolution, and the behaviour of the biosphere in terms of the laws of physics, and those who wished to make the success of evolution just another manifestation of the mysteriousness of living things.'<sup>207</sup>

Cairns' tone and presentation resulted in the critics' perception of him as one of these romantics, whereas Hall's style made him appear more compatible with the reductionists.

Despite Hall's more moderate approach his mechanistic theories had significant impact in scientific terms, and he came the closest of any of the advocates to achieving a degree of acceptance for directed mutation. In 1990, Hall formally proposed 'hypermutation' as a mechanistic theory for directed mutation.<sup>208</sup> Building upon his earlier strong and weak versions of hypermutation he proposed a model in which a proportion of the cells in a bacterial colony might enter a hypermutable state during which their genome rapidly and randomly mutates. Any one of that population experiencing a beneficial mutation might exit stationary phase, and reproduce to form a clone of adapted cells. Hypermutation of a sub-population provided a means by which an adaptive response could be mounted to environmental stress, without relying on an assumption of non-random mutation. Cairns had presented a strong model of directed mutation based on cell choice and reverse transcription; Hall added to that this weaker model of random hypermutation as an evolved strategy for rapid adaptation in hostile environments.

Hall's hypermutation theory was popular amongst the protagonists of directed mutation, but also popular as an explanation amongst those critics troubled by the Lamarckian associations emphasised by Cairns. As Hall pointed out: 'Because the randomness of spontaneous mutation forms such a basic part of our view of biological processes, most of us may be more comfortable with an underlying random

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<sup>207</sup> Cairns, J., Overbaugh, J. & Miller, S. (1988) p.145

<sup>208</sup> Hall, B. (1990a)

mechanism than with a directed one.'<sup>209</sup> Although, he adds: 'We should be cautious, however, about rejecting the notion of 'directed' mutations simply because it makes us more comfortable to do so.'<sup>210</sup> Hall still maintained that hypermutation might focus the mutational effort on those specific gene loci most likely to yield an adaptive mutation; thus making the process directed, but not in the exact sense that Cairns had anticipated. The site specific hypermutation concept was agreeable to the dedicated directed mutationists (Patricia Foster advocated hypermutation after 1993), whereas genome-wide hypermutation theory, or hypermutation of a sub-population of bacterial cells, were more popular with individuals who wished to express the directed mutation phenomena within the bounds of Neo-Darwinian theory.

Repetitions of the directed mutation observations, and the extension of the original *E. coli* assays to show the process in other unicells, did not help the debate reach resolution. By the early 1990s dozens of mechanisms for directed mutation had been proposed, from the most Lamarckian to the almost wholly Neo-Darwinian. Cairns' Harvard team had proposed the most controversial and Lamarckian mechanism in their first paper, suggesting that the selective conditions instructed the genome to mutate specifically and that reverse transcriptase enabled the process.<sup>211</sup> Davis had presented a similarly strong model, suggesting that the agent of selection itself engaged with the DNA, inducing transcription and creating regions of single stranded DNA. Those single strand regions are vulnerable to damage, and so mutation would be localised at sites specifically related to the selective conditions.<sup>212</sup> Stahl<sup>213</sup>, and later Boe<sup>214</sup>, had proposed the 'slow repair' models. These stated that under conditions of nutritional stress the normal DNA repair enzymes might be slow to act and so might allow mutations to accumulate at an elevated rate in stressed colonies. In the strong form these DNA errors are localised at sites related to the stress conditions, in the weaker model the DNA errors would be genome wide and more random. Hall had rejected these models in favour of the trial and error models

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<sup>209</sup> Hall, B. (1990a) p.15

<sup>210</sup> Hall, B. (1990a) p.15

<sup>211</sup> Cairns, J., Overbaugh, J. & Miller, S. (1988)

<sup>212</sup> Davis, B. (1989) Transcriptional bias: a non-Lamarckian mechanism for substrate-induced mutations. *Proc. Natl. Acad. Sci. USA*, 86: 5005-5009.

<sup>213</sup> Stahl, F. (1988)

<sup>214</sup> Boe, L. (1990) Mechanism for induction of adaptive mutation in *Escherichia coli*. *Molecular Microbiology*, 4: 597-601.



related to the various modes of hypermutation.<sup>215</sup> Connolly and Winkler<sup>216</sup>, and Ninio<sup>217</sup> had suggested that a special kind of bacterial cell, known as a transient mutator, might underlie directed mutation. These cells have a naturally high mutation rate due to having either a mutator gene, or a faulty repair gene. Those cells would contribute the mutations that the population required to overcome the environmental challenge. However, experiments failed to confirm that any of these mechanisms was responsible for directed mutation, but also failed to eliminate any of them.

The issue of the specificity of the mutations arising under selection remained hotly contested. In the early 1990s it remained unclear whether the mutations that accumulated were only those that had some utility, or whether neutral mutations were also occurring alongside the adaptive mutations. This issue was at the heart of the debate, since, the more specifically the mutations were directed to the environmental demands, the more Lamarckian the interpretation of the phenomenon must be. In 1992 Foster remarked that: 'Perhaps the most astounding aspect of directed mutation is, of course, that it is 'directed', i.e., only those mutants that are selected for arise in the population. Cells under selection are not, apparently, accumulating useless mutations.'<sup>218</sup> She lamented that: 'Unfortunately, this is the aspect that is least supported by experimental evidence.'<sup>219</sup>

One key problem had been identifying a truly neutral mutation that could be used as a control study. The accumulation of the Valine resistance mutation had been examined several times<sup>220</sup>, and it had been concluded that this non-selected mutation was not arising at the same rate as the beneficial mutations elsewhere in the genome. So, it appeared that the mutations arising were specifically directed to the demands of the environment. However, it had been argued that Valine resistance might be either a special case or not fully neutral.<sup>221</sup> Similar concerns about neutrality, and the

<sup>215</sup> Hall, B. (1990a); Hall, B. (1991b)

<sup>216</sup> Connolly, D. & Winkler, M. (1989) Genetic and physiological relationships among the *miaA* gene, 1-methylthio-N<sup>6</sup>-( $\Delta^2$ -isopentenyl)-adenosine tRNA modification, and spontaneous mutagenesis in *Escherichia coli* K-12. *Journal of Bacteriology*, 171: 3233-3246.

<sup>217</sup> Ninio, J. (1991) Transient mutators: a semiquantitative analysis of the influence of translation and transcription errors on mutation rates. *Genetics*, 129: 957-962.

<sup>218</sup> Foster, P. (1992) Directed mutation: between unicorns and goats. *Journal of bacteriology*, 174: 1711-1716. p.1712

<sup>219</sup> Foster, P. (1992) p. 1712

<sup>220</sup> Cairns, J., Overbaugh, J. & Miller, S. (1988); Hall, B. (1988); Hall, B. (1990a)

<sup>221</sup> Stahl, F. (1990) If it smells like a unicorn.... *Nature*, 346: 791; Drake, J. (1991) Spontaneous mutation. *Annual Review of Genetics*, 25: 125-146.

advantages of genetic intermediates, plagued studies of double adaptive mutations. Cairns' team had cited adaptations that require a double mutation as likely cases of directed mutation<sup>222</sup>, since the chance of double mutation being achieved by chance alone, as Foster has put it, are 'unreasonably low'.<sup>223</sup> Hall had provided two dramatic examples of double mutations arising at frequencies far in excess of that predicted by chance or random mutation.<sup>224</sup> However, it was argued that even these results might be the result of ill-judged neutrality, or the cryptic advantage of certain intermediates.<sup>225</sup>

So, in the early 1990s it was clear that the directed mutation phenomenon existed in bacteria, and also in the unicell yeast. The appearance of direction occurred in relation to several different kinds of mutation, including point, substitution and frameshift. And even adaptive changes relying on the simultaneous achievement of two very rare mutations seemed to occur quite commonly under selective conditions. However, despite these confirmations, the criticisms of directed mutation persisted and retained the quality that they had taken on in the late 1980s. The issue of specificity was key, and the interpretation the 'directed' aspect of the phenomenon was very much open to debate.<sup>226</sup> Luria and Delbruck's fluctuation analysis retained significance for the critics of directed mutation. The onus remained on the supporters of directed mutation to demonstrate that that precedent was not relevant.

The strongest and perhaps most enduring critics of directed mutation were evolutionary biologists Richard Lenski and John Mittler. They were critical of Cairns' rhetoric and dismayed at the invocation of Lamarckism. As early as April 1989, Lenski co-authored a response to the directed mutation observations with Francisco Ayala, an ardent Neo-Darwinian. They proposed Darwinian explanations for the apparent anomaly, and explained away the contentious factors that Cairns had highlighted. In relation to Cairns' invocation of Lamarckism they stated:

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<sup>222</sup> Cairns, J., Overbaugh, J. & Miller, S. (1988)

<sup>223</sup> Foster, P. (1992) p.1712

<sup>224</sup> Hall, B. (1988)

<sup>225</sup> Mittler, J. & Lenski, R. (1992) Experimental evidence for an alternative to directed mutation in the *bgl* operon. *Nature*, 356: 446-448.

<sup>226</sup> Benson, S., Decloux, A. & Munro, J. (1991) Mutant bias in non-lethal selections results from selective recovery of mutants. *Genetics*, 129: 647-658. In which the authors propose several alternative causes of apparent specificity in bacteria adaptation.

‘Cairns *et al.*...assert that directed mutation in bacteria (if it is demonstrated to exist) ‘could in effect provide a mechanism for the inheritance of acquired characteristics’. We disagree with this claim; we also view it as potentially harmful in that it may seem to give credence to prescientific claims that have been thoroughly disproved.’<sup>227</sup>

They further blast Cairns’ Lamarckism saying: ‘...even if directed mutation were demonstrated in bacteria, this would not support in any manner whatsoever the traditional notions of Lamarck and his followers concerning the acquisition of adaptive characteristics.’<sup>228</sup> They conclude that any invocation of Lamarckism in relation to directed mutation in bacteria ‘may perpetuate mistaken beliefs concerning heredity that are still widely held outside scientific circles.’<sup>229</sup>

In 1990 Richard Lenski first co-authored with John Mittler on directed mutation.<sup>230</sup> Their association represents one of the most stable in the core-set, alongside that of John Cairns and Patricia Foster. The authors presented evidence against the Harvard interpretation of the directed mutation anomaly, and offered alternative explanations for the findings.<sup>231</sup> In 1993, they published a lengthy criticism of directed mutation.<sup>232</sup> In that paper they reiterated the principles of Neo-Darwinian theory that precluded Cairns’ observations and gave a detailed description of Luria and Delbruck (1943) as evidence against the reliability of the directed mutation observations. The authors did not deny that a phenomenon had been observed in starved bacterial populations, but rather they suggested it had been misinterpreted. The main body of that review article describes the standard Neo-Darwinian explanations that might account for apparent directed mutation. Lenski and Mittler presented a summary of the various mechanisms that had been proposed by that time. These included:

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<sup>227</sup> Lenski, R., Slatkin, M. & Ayala, F. (1989) p.2777

<sup>228</sup> Lenski, R., Slatkin, M. & Ayala, F. (1989) p.2778

<sup>229</sup> Lenski, R., Slatkin, M. & Ayala, F. (1989) p.2778

<sup>230</sup> Mittler, J. & Lenski, R. (1990) New data on excisions of Mu from *E. coli* MCS2 cast doubt on directed mutation hypothesis. *Nature*, 344: 173-175.

<sup>231</sup> Mittler, J. & Lenski, R. (1990); Mittler, J. & Lenski, R. (1992)

<sup>232</sup> Lenski, R & Mittler, J. (1993a)

1. Reverse transcription of specific beneficial mRNA.<sup>233</sup>
2. Reverse transcription of random mRNA.<sup>234</sup>
3. Achievement of adaptation by mutation through slow or error prone mismatch repair.<sup>235</sup>
4. Adaptation through hypermutation of either the whole genome or specific useful areas of the genome.<sup>236</sup>

Lenski and Mittler addressed these proposed mechanisms and described how each, while showing a particular capacity in bacteria to achieve adaptation in extreme conditions, could be re-described to remove the element of direction, and more importantly, the notion of cell 'choice'. They suggested that the random reverse transcription of mRNA, increased mismatch repair and hypermutation need only rely on random genetic processes compatible with Neo-Darwinian theory. The mutations were not specific, they only appeared so on account of the re-growth only of those mutants which were successfully adapted by their mutation to the environment conditions. The underlying principle therefore remains random mutation coupled to natural selection as Neo-Darwinism demands. The authors acknowledged that non-Darwinian interpretations had been offered by others, but stated simply that: 'Various molecular models have been proposed that might explain these directed mutations, but the models have not been confirmed.'<sup>237</sup>

In their 1993 paper, Lenski and Mittler made the lack of a demonstrable mechanism for directed mutation the greatest burden of proof for the advocates of directed mutation. Foster had already acknowledged the obligation to reveal mechanism<sup>238</sup>, and just four months earlier Foster and Cairns had published a paper exploring a further three potential mechanisms for directed mutation; two strong directed models, and one weaker random model.<sup>239</sup> From the start, each of Cairns' and Hall's papers had included the details of hypothesised mechanisms for directed

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<sup>233</sup> As proposed in Cairns, Overbaugh & Miller (1988) as the strong version.

<sup>234</sup> As proposed in Cairns, Overbaugh & Miller (1988) as the weak version.

<sup>235</sup> As proposed in Stahl, 1988

<sup>236</sup> Barry Hall proposed, and was the strongest advocate of, hypermutation in its various forms from 1989.

<sup>237</sup> Lenski, R. & Mittler, J. (1993a) p.188

<sup>238</sup> Foster, P. (1992)

<sup>239</sup> Foster, P. & Cairns, J. (1992) Mechanisms of directed mutation. *Genetics*, 131: 783-789.

mutation, demonstrating their own appreciation that such detail was required alongside observation. In 1993, their appreciation of that requirement was heightened as Lenski and Mittler explicitly criticised the deficit of mechanism in directed mutation theory. After 1993, the demonstration of mechanism was the primary focus of both advocates and adversaries in this debate.

The significance allotted to the identification of mechanism by the critics and supporters of directed mutation relates to the recognised processes for acquiring credibility. The perceived need to propose a mechanism on the part of the advocates of directed mutation, most particularly in the initial phases, led to publications becoming very speculative. This quality of course only served to foster the critics' claims that reports of directed mutation were speculative and unsubstantiated. However, Cairns was apparently unperturbed by their criticism and responded stating that it was '...too selective and partisan to be useful.'<sup>240</sup>

In 1993, Patricia Foster, of Boston University, summarised the evidence for directed mutation with her own review article of the findings to date.<sup>241</sup> Support for directed mutation was becoming diffuse, as advocates dispersed their allegiances amongst the range of strong and weak models that had become available. Cairns and Foster stood almost in isolation as proponents of the stronger models. Critics had laid out a programme for further investigation to serve their agenda of Neo-Darwinian adherence<sup>242</sup>, and in her review Foster countered that by offering research guidance for supporters of the theory.

By 1993 Foster had already co-authored two directed mutation papers with John Cairns.<sup>243</sup> She had been involved with directed mutation from the beginning, being acknowledged in the 1988 Harvard paper for reading of manuscripts in preparation<sup>244</sup>. Foster had been a graduate student in Cairns' laboratory and had been recruited to the debate during her studentship.<sup>245</sup> She eventually became the most

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<sup>240</sup> Cairns, J. (1993) Directed mutation. *Science*, 260: 1221-1223. p.1221

<sup>241</sup> Foster, P. (1993)

<sup>242</sup> See Lenski, R. & Mittler, J. (1993a) in which the authors describe experiments that might demonstrate their Darwinian interpretation of directed mutation.

<sup>243</sup> Cairns, J. & Foster, P. (1991) Adaptive reversion of a frameshift mutation in *Escherichia coli*. *Genetics*, 128: 695-701; Foster, P. & Cairns, J. (1992)

<sup>244</sup> Cairns, J., Overbaugh, J. & Miller, S. (1988)

<sup>245</sup> Foster had worked throughout the early 1980s on bacterial mutagenesis with her Harvard colleagues. See: Foster, P., Eisenstadt, E. & Cairns, J. (1982) Random components in mutagenesis. *Nature*, 299: 365.

persistent and prolific member of the advocates' section of the core-set, and John Cairns now acknowledges her as the leading authority on directed mutation.<sup>246</sup> In 1991, Foster and Cairns had focused on a reverse transcriptase based mechanism, asserting the strong model of directed mutation.<sup>247</sup> They reported results of experiments demonstrating that the presence of the RecA gene was required for directed mutation events to be observed in bacterial populations. This observation showed that DNA was being replicated during stationary phase mutations and added credence to Cairns' theory of multiple variable mRNA generation and reverse transcription as the principle underlying directed mutation. In 1992, the two authors had pursued the strong models addressing two hypotheses: i) the selective conditions 'instruct' the cell which DNA sequence changes to create and ii) the selective conditions induce transcription of the gene encoding the relevant protein, and the transcription is inherently mutagenic.<sup>248</sup> Foster had become Cairns' strongest ally. Like Hall, she supported the claim that an apparently directed process appeared in bacterial mutation. But, going beyond Hall, she also supported Cairns' contentious interpretation of the anomaly, and its implications for evolutionary theory.

In 1993, Foster did not shy from framing the debate as a conflict between Neo-Darwinians and New Lamarckians.<sup>249</sup> The rhetorical approach that Cairns had adopted as part of the authority struggle with the evolutionary biologists is mirrored in Foster's contributions. She acknowledges a molecular biological problem alongside an evolutionary biological problem and, like Cairns, she extrapolates from the Petri dish to the general phenomena of evolution. Foster's willingness to describe evolutionary outcomes made her Cairns' ally in his struggle for the extension of molecular biologists' authority.

In 1993, Foster identified the Lamarckian implications of Cairns' 1988 paper as the major cause of the quality and quantity of response. Essentially, Foster identified the Lamarckism versus Darwinism element of the debate as a primary force in its perpetuation. She acknowledges that, although the criticisms levelled at the directed mutation publications tend to be of a methodological nature, it seems that the

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<sup>246</sup> John Cairns (personal communication 06/04)

<sup>247</sup> Cairns, J. & Foster, P. (1991)

<sup>248</sup> Foster, P. & Cairns, J. (1992) p.785

<sup>249</sup> Foster, P. (1993)

motivation for raising the criticisms stems from antagonism towards the Lamarckian principles linked to the anomaly. As Foster says: 'Even in peer reviewed articles, scientists have exhibited a surprising fervour, verging on the religious, in debating this issue.'<sup>250</sup> Foster also identifies the role that dogma had played in the criticisms, stating that: 'Features of Neo-Darwinian molecular genetics that have been credited as fundamental to our understanding of evolutionary genetics exist as an additional challenge to the directed mutation community'.<sup>251</sup> The burden for the directed mutation researchers was not just to prove the existence and reliability of their initial observations, but also to show that these findings were a substantial opposition to accepted principles of evolutionary theory. In terms of the conflict of directed mutation with the Luria and Delbruck<sup>252</sup> findings, Foster says that the pre-existing fluctuation test data had been a serious impediment, since to 'criticise it seems almost sacrilegious'.<sup>253</sup>

As Foster wrote this article, the phenomenon of 'directed' mutation was being discussed as a reality of bacterial stationary phase. The specificity of that 'direction' and the reliance of the phenomenon on non-random mutation nevertheless remained contentious. The possible Lamarckian interpretation of the theory had gained no favour with the critics, and so the core-set remained sharply polarised on the issue of the potential implications of directed mutation for evolutionary theory. Foster's 1993 review, and Lenski and Mittler's 1993 article, can be viewed as marking the end of the phase of publications asserting and reasserting the anomaly of directed mutation, and as the beginning of the protracted theoretical debate that has retained momentum to the present day. On the issue of the existence of an evolved adaptive mutational process in bacteria the core-set had come to some agreement; that process had been demonstrated. On the issue of strong versus weak interpretations, the existence of the phenomenon outside bacteria, and the interpretation of the significance for evolutionary theory, the advocates and adversaries remained sharply delineated.

In terms of its boundary work function, Foster's 1993 review represents a regrouping device for the protagonists of directed evolution. It reasserted the terms of

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<sup>250</sup> Foster, P. (1993) p.468

<sup>251</sup> Foster, P. (1993)

<sup>252</sup> Luria, S. & Delbruck, M. (1943)

<sup>253</sup> Foster, P. (1993) p.470

the authority challenge to evolutionary biology from molecular biology, by re-emphasising the evolutionary implications of the molecular observations. The review takes stock of achievements and failures, at a time when the challenge for advocates of directed mutation was shifting towards the requirement to demonstrate the molecular mechanism for directed mutation and extend directed mutation assays to other organisms. Like Cairns, Foster invokes Lamarckism to assert that challenge and to frame the dissent. Her article focuses on those elements of the debate that had kept the core-set so divided, highlighting the need for a concerted effort towards consensus amongst the advocates.

In 1992, Franklin Stahl had published the second of his three review articles on directed mutation and had acknowledged the requirement for a focus on mechanism, stating that: 'Some critics appear to be blindly sceptical of the demonstrations offered in support of the view that cells can mutate in a directed way. By failing to provide a proven (or even attractive) hypothesis, the recent work of Foster and Cairns (1992) is unlikely to quiet such detractors.'<sup>254</sup> In 1993, Foster's tone remained optimistic, as she pointed out that their adversaries were equally lacking effective counter arguments for the mechanisms that had been suggested.

After 1993, theoretical problems continued to mount alongside an absence of any significant new data from further bacterial research. Evidence for a mechanism was lacking, largely due to the absence of any technologies or methodologies that might be able to visualize a molecular mechanism of the type that had been hypothesised. The strong version of directed mutation was heavily laden with speculation, and necessarily so. Yet the speculative accounts could not be abandoned, since critics continued to emphasise the lack of mechanism as the greatest hurdle for the advocates of directed mutation. The aspect of the debate that dealt with the molecular biological problem had lost momentum as a result. The same criticisms resurfaced time and again, and the same issues concerning experimental design and interpretation were still being discussed.<sup>255</sup> A turning point had arrived in the

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<sup>254</sup> Stahl, F. (1992) p.867

<sup>255</sup> As late as 1994 Foster and Cairns were addressing the apparent refutation of directed mutation by Luria and Delbruck's fluctuation test, and were still answering the methodological criticisms of their opponents. See: Foster, P. & Cairns, J. (1994) The occurrence of heritable Mu excisions in starving cells of *E. coli*. *EMBO*, 13: 5240-5244.



negotiation phase. Stagnation and circular argument could only be avoided by a change of approach to suit the obligation to discover mechanism

Directed mutation received this much-needed boost in the mid-1990s when biologist Susan Rosenberg joined the debate. Like Foster she would be a persistent advocate and a prolific author. Also, like Foster, she would recruit and co-author with many other researchers throughout the 1990s and early 2000s. In 1994 Rosenberg produced three papers on directed mutation.<sup>256</sup> These confirmed the role of recombination in adaptive mutation, and linked that process to hypermutation in certain strains of bacteria. Rosenberg pursued mechanistic explanations for directed mutation, taking on that key responsibility of advocacy in this period. Foster and Rosenberg had worked simultaneously, but independently, during 1993-1994 to collect qualitative data demonstrating directed mutation. In 1994, they presented that data in a pair of papers in the July issue of the journal *Science*.<sup>257</sup> The two authors presented sequence analyses that showed stressed bacterial genomes undergoing novel mutational processes to produce specific kinds of mutation. By refining the focus of their study on the genome, rather than the bacterial colony, they bypassed all the methodological criticisms concerning counting and discreet cell growth and death that had been a major part the debate from the beginning. Their quantitative approach was exactly what the debate required to move beyond the initial phase of repetition and negotiation.

Franklin Stahl said: ‘with essentially one [stroke], this qualitative observation rules out the possibility that the whole thing was an unintentional fake.’<sup>258</sup> In a review appearing alongside the two papers Elizabeth Culotta wrote that the papers showed that ‘something exceptional was happening in Cairns’ experiments’.<sup>259</sup> She reported that even Richard Lenski was now convinced that some unusual mutational process was at work, at least in the experimental system.<sup>260</sup> Meanwhile, Hall published a

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<sup>256</sup> Harris, R., Longerich, S. & Rosenberg, S. (1994) Recombination in adaptive mutation. *Science*, 264: 258-260; Rosenberg, S. *et al* (1994) Adaptive mutation by deletions in small mononucleotide repeats. *Science*, 265: 405; Rosenberg, S. (1994) In pursuit of a molecular mechanism for adaptive mutation. *Genome*, 37: 893-899.

<sup>257</sup> Rosenberg, S. *et al* (1994); Foster, P. & Trimarchi, J. (1994) Adaptive reversion of a frameshift mutation in *Escherichia coli* by simple base deletions in homopolymeric runs. *Science*, 265: 407-409.

<sup>258</sup> Quoted from personal communication in Culotta, E. (1994) A boost for ‘adaptive’ mutation. *Science*, 265: 318-319.

<sup>259</sup> Culotta, E. (1994) p.318

<sup>260</sup> Culotta, E. (1994)

forceful article, restating that directed mutations are ‘immediately, rather than only potentially, advantageous’ (i.e. they are specific) and attacking Lenski and Mittler’s (1992) experiments and conclusions.<sup>261</sup> Negotiation was back on.

However, neither of the two papers addressed the specificity of the mutations, and so avoided the most contentious element of the debate. Regarding specificity, Culotta reported that Lenski and others remained totally unmoved. She acknowledges that by 1994 the issue of specificity was still wide open. Some authors claimed to have demonstrated specificity, others claimed to have shown neutral mutations accumulating alongside the adaptive. In their 1994 papers, Rosenberg and Foster both invoke mechanisms that rely on a degree of random mutation, essentially weaker models of directed mutation. As Foster puts it: ‘The question becomes not whether the process is random, but where does the randomness appear.’<sup>262</sup> Culotta reports that Lenski and Mittler see this move towards the weaker models as a sign that their opponents have retreated from ‘anything that challenges evolutionary dogma’.<sup>263</sup> Mittler concludes that: ‘The debate is getting down to bacterial physiology in stationary phase now. It’s an important area of investigation – but it’s unlikely to be Lamarckian.’<sup>264</sup>

In Lenski and Mittler’s view the position adopted by Foster and Rosenberg in 1994 signalled the end of the challenge to evolutionary biology. Their advocacy of weaker models of directed mutation reduced the significance of the anomaly for evolutionary biologists. Mittler’s observation that the debate ‘was getting down to bacterial physiology’ was really an observation that the debate had shifted from the boundary between evolutionary biology and molecular biology, and moved back inside the territory delineated for molecular biology. As such, its authority challenge had subsided, and Lenski and Mittler’s defence of the authority of evolutionary biology, and the Neo-Darwinian orthodoxy that underpinned it, became redundant. Although Hall had questioned their 1992 experiments and conclusions in 1994, the authors did not respond in print. Hall’s challenge came from within ‘bacterial physiology’ and a response related to the defence of authority was thus not required.

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<sup>261</sup> Hall, B. (1994) On alternatives to selection-induced mutation in the Bgl operon of *Escherichia coli*. *Mol. Biol. Evol.*, 11: 159-168.

<sup>262</sup> Foster, P. & Trimarchi, J. (1994)

<sup>263</sup> Culotta, E. (1994)

<sup>264</sup> Reported in Culotta, E. (1994) from personal communication with John Mittler.

Lenski and Mittler's commentary reported in Culotta's review marks their exit from the debate, and the controversy core-set. By focussing on technical experimental detail, and removing speculative extrapolations (as their critics had demanded) Foster and Rosenberg had been forced to retreat from the authority contest that Cairns' approach had framed. By implying the need to retreat from that boundary as a 'scientific' obligation for the molecular biologists, the evolutionary biologists had achieved the boundary protection that retained their monopoly.

Thus, during 1994 the fortunes of the two aspects of the debate divided dramatically. The molecular biological anomaly of directed mutation achieved a degree of acceptance, although only in abstraction from the implications for evolutionary biology that it had been originally bound to. Directed mutation research was acceptable, but only provided it was not attached to an authority challenge to evolutionary biology. A new phase of debate began in which the strong and weak models of directed mutation were explored in qualitative detail. The journal debate had been re-enlivened, but without the dual aspect that had originally characterised it. This new negotiation was conducted largely within the molecular biological community, amongst its group of specialists. Explicit attention to the Lamarckian versus Darwinian element of the debate declined in the journals. Evidence for the strongest and most Lamarckian models was scarce, and so negotiation in journal treatments stagnated on that issue. The core-set that had formed in service to that debate began to decay. Lenski and Mittler perceived that their defence was more or less complete. Lenski betrays what had been the two authors' agenda of boundary work for defence, using the word 'retreat' to describe the directed mutation advocates' shift of attention to weaker models.

The Lamarckism versus Darwinism aspect of the debate did not disappear altogether, or reach resolution. It remained understood that directed mutation, whatever its cause or nature, would have a bearing on evolutionary theory; it was mentioned, but remained tacit and secondary to the molecular debate. Lenski and Mittler's exit from the debate did not imply resolution, or that they perceived the negotiation to have been completed. Rather, without explicit attention to the challenge of directed mutation to Neo-Darwinism, defence of orthodoxy of the kind that they had offered was no longer required or appropriate. The authority challenge

had abated, and so had the need for defence of authority. By 1995 only Open University professor of biology Brian Goodwin echoed the hard line, that 'neo-Darwinism had failed as an evolutionary theory' in light of the 'evidence for a role of directed mutation in adaptive response' and the discovery that 'genes can evidently respond to environmental circumstances by non-random adaptive mutation'.<sup>265</sup>

Susan Rosenberg drew the advocates towards technical, qualitative investigation of directed mutation. Her 1994 papers demonstrated attention to detail and focussed on methodology and the provision of data. Foster followed suit, and pursued an investigation of the population dynamics and mutational specificity of bacterial colonies during Lac<sup>+</sup> adaptive reversion. The results of that work she reported in great technical detail, without accompanying commentary on the broader evolutionary implications.<sup>266</sup> In 1995, Galitski and Roth began an investigation of the role of the F-plasmid (extranucleic genetic material in bacteria) in adaptive mutation, concluding that some recombination, and some plasmid transfer between bacteria, were probably involved in directed mutation.<sup>267</sup> These authors described how bacteria use certain fertility factors to control the degree to which they conjugate with one another and exchange plasmid material in a kind of sexual reproduction. They suggest that less 'fertile' bacteria achieve less adaptive reversion during selection for lactose reversion, and that therefore, adaptive mutation is related to the primitive sexual reproduction of bacteria. In the same issue of *Science* biologist Radicella's team confirmed that selection induced mutation and plasmid transfer are inseparable.<sup>268</sup> Alongside their papers molecular biologist James Shapiro provided a review article summarising the findings on the role of biochemical process in adaptive mutation. He stated that the demonstration of these kinds of evolutionary strategies '...moves mutation beyond the realm of 'blind' stochastic events and provides a mechanistic basis for understanding how biological requirements can feed back into genome structure'.<sup>269</sup> Even after seven years of debate, Shapiro still gave

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<sup>265</sup> Goodwin, B. (1995) Neo-Darwinism has failed as an evolutionary theory. *The THES*, 19.05.95.

<sup>266</sup> Foster, P. (1994) Populations dynamics of a Lac- strain of *Escherichia coli* during selection for lactose utilization. *Genetics*, 138: 253-261.

<sup>267</sup> Galitski, T. & Roth, J. (1995) Evidence that F-plasmid transfer replication underlies apparent adaptive mutation. *Science*, 268: 421-423.

<sup>268</sup> Radicella, J., Park, J. & Fox, M. (1995) Adaptive mutation in *Escherichia coli*: a role for conjugation. *Science*, 268: 418-420.

<sup>269</sup> Shapiro, J. (1995) Adaptive mutation: who's really in the garden. *Science*, 268: 373-374.

space in that paper to statements of the distinction between the adaptive mutation assays and the work of Luria and Delbruck. Shapiro had noted the limitations of Luria and Delbruck's lethal selection methodology, and had published on this in 1984, providing useful precedent for the directed mutation claims against the Fluctuation Test.<sup>270</sup> Foster confirmed that adaptive reversion did appear to be linked to the conjugal functions of bacteria, but added that the process did not seem to rely on actual conjugation events.<sup>271</sup> This implied that adaptive mutations occurred during stationary phase DNA synthesis, which was occurring in cells preparing to transfer plasmid material. Again, this material was presented without commentary on broader implications.

As Mittler had predicted, it seemed that the debate really was 'getting down to bacterial physiology', with an explicit focus on problems related to molecular factors. As Shapiro had put it in April 1995, 'adaptive mutation has returned to the mainstream of molecular genetics'.<sup>272</sup> Cairns also contributed to the discussion of the role of plasmid transfer in directed mutation, in a letter to the journal *Science*.<sup>273</sup> Following the new trend in the debate Cairns kept his comments technical, and focussed on mechanism. Although, he did complain that Radicella<sup>274</sup> and Galitski and Roth<sup>275</sup> had concluded on some issues that they had not tested, thus overemphasising the role of plasmid transfer, and that, Galitski and Roth cited Foster 'seeking support', but in a misleading way. This approach and tone demonstrates the legacy of Cairns' role as principal advocate; he retained responsibility for ensuring fairness, even without explicitly pursuing the broader aspect of the debate.

This shift towards molecular debate suited Barry Hall, who had always avoided the broader evolutionary speculations. In 1995, Hall continued his technical investigations of bacterial mutation, and began to move directed mutation research into a new area, looking at the possible role of the process in the emergence of some

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<sup>270</sup> Shapiro, J. (1984) Observations on the formation of clones containing *araB* – *lacZ* cistron fusion. *Mol. Gen. Genet*, 194: 79-90.

<sup>271</sup> Foster, P. & Trimarchi, J. (1995) Adaptive reversion of an episomal frameshift mutation in *Escherichia coli* requires conjugal functions, but not actual conjugation. *Proc. Natl. Acad. Sci., USA*, 92: 5487-5490.

<sup>272</sup> Shapiro, J. (1995) p.374

<sup>273</sup> Cairns, J. (1995a) Adaptive mutation and sex. *Science*, 269: 289.

<sup>274</sup> Radicella, J., Park, J. & Fox, M. (1995)

<sup>275</sup> Galitski, T. & Roth, J. (1995)

tumours.<sup>276</sup> This investigation, interestingly, brought the debate back into contact with its origins; Cairns' initial observations of directed mutation arose from the context of his attention to mutation as a factor in carcinogenesis.<sup>277</sup> Meanwhile, Foster and Rosenberg (independently) continued to carry out technical investigations, co-authoring with a variety of specialists, and investigating the roles of recombination and DNA repair in more detail.<sup>278</sup> Each of their contributions added to the understanding of the molecular processes underlying the appearance of adaptive mutation in selective cultures.

After 1995, Hall maintained his focus on hypermutation, and remained the most prolific author on this theory. Cairns' original strong theory of an environment-directed reverse transcriptase mechanism had not been demonstrated, and it seemed much more likely that one of the weaker models (that relied upon a variety of more random mutational processes) would explain the generation of the mutations. Foster<sup>279</sup> and Rosenberg began to adopt Hall's notion of genome-wide hypermutation in their research. Rosenberg led a research team investigating the location of hypermutation within the evolving bacterial colony. In 1997, they reported that hypermutation occurred in a sub-population of cells within the colony.<sup>280</sup> That is, some cells in a colony had the capacity to enter a transient state of hypermutation during physiological stress, and effectively sacrifice themselves in an effort to achieve adaptive mutation.<sup>281</sup>

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<sup>276</sup> Hall, B. (1995a) Adaptive mutations in *Escherichia coli* as a model for the multiple mutational origins of tumours. *Proc. Natl. Acad. Sci., USA*, 92: 5669-5673; Hall, B. (1995b) Evolutionary potential of the *ebgA* gene. *Mol. Biol. Evol.*, 12: 514-517

<sup>277</sup> Cairns' describes the relationship between directed mutation and cancer studies in: Cairns, J. (1998) Mutation and cancer: the antecedents to our studies of adaptive mutation. *Genetics*, 148: 1433-1440.

<sup>278</sup> Foster, P. *et al* (1995) Proofreading-defective DNA polymerase II increases adaptive mutation in *Escherichia coli*. *Proc. Natl. Acad. Sci., USA*, 92: 7951-7955; Longerich, S., Rosenberg, S. *et al.* (1995) Adaptive mutation sequences reproduced by mismatch repair deficiency. *Proc Natl. Acad. Sci., USA*, 92: 12017-12020; Foster, P. Trimarchi, J. & Maurer, R. (1996) Two enzymes, both of which process recombination intermediates, have opposite effects on adaptive mutation in *Escherichia coli*. *Genetics*, 142: 25-37.

<sup>279</sup> Foster applies the notion of hypermutation in: Foster, P. (1997) Nonadaptive mutations occur on the F' episome during adaptive mutation conditions in *Escherichia coli*. *Journal of Bacteriology*, 179: 1550-1554.

<sup>280</sup> Torkelson, J., Rosenberg, S. *et al* (1997) Genome-wide hypermutation in a sub-population of stationary-phase cells underlies recombination-dependent adaptive mutation. *EMBO*, 16(11): 3303-3311.

<sup>281</sup> Theories of adaptive mutation relying on hypermutation in a sub-population of the colony were supported at this time by the discovery by other authors that 'mutator' type bacterial cells were much more common in colonies than had previously been considered. These individual bacteria have a specific tendency to mutate much faster and more diversely than other cells in the population. These

Glimpses of the broader dissent remained. Galitski and Roth had moved towards the stronger interpretation of directed mutation and had begun to seek the 'general phenomenon' which they suggested would have 'evolutionary implications'.<sup>282</sup> They went as far as to say: 'Since organisms typically exist under relatively adverse or selective conditions, the potential importance of adaptive mutability is great', which echoed Cairns' earlier broad statement that '...cells may have mechanism for choosing which mutations will occur.'

In 1997, the contentious issue of specificity reappeared. Hypermutation theory implied that neutral mutations should appear alongside selected mutations. Earlier research had showed time and again that adaptive mutations seemed to arise preferentially, and that neutral mutations, such as Valine resistance, were not accumulated at the same rate.<sup>283</sup> In 1997, Hall addressed specificity, and confirmed that in the *ebg* system adaptive mutations arose without concurrent accumulation of neutral traits.<sup>284</sup> This report moved directed mutation back towards the stronger models, with their focus on environment direction of the specific quality of mutation. In contrast to the predictions of hypermutation, Hall argued that it appeared that neither carbon nor amino acid starvation were generally mutagenic. He suggested that the *ebg* gene of *E. coli* provided a system in which specificity could be studied without invoking the problems of neutrality interpretation that had plagued other investigations. In the conclusion of that paper, Hall went on to demonstrate that specificity might be commensurate with hypermutation, despite its counter-intuitive appearance. He suggested that the appearance of specificity might be created by an underlying process of 'selective recapture', in which mutations are occurring abundantly and randomly in the population, but only those that are specific and

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mutator types would constitute the sub-population invoked by directed mutation advocates in the mid-1990s. Leclerc *et al* (1996) High mutation frequencies among *E. coli* and *Salmonella* pathogens. *Science*, 274: 1208-1211; Grady, D (1996) Quick-change pathogens gain an evolutionary edge. *Science*, 274: 1081.

<sup>282</sup> Galitski, T. & Roth, J. (1996) A search for the general phenomenon of adaptive mutability. *Genetics*, 143: 645-659.

<sup>283</sup> Specificity is reported in: Cairns, J., Overbaugh, J. & Miller, S. (1988); Foster P. (1992); Hall, B. (1988); Hall, B. (1990a); Hall, B. (1990b); Cairns, J. & Foster, P. (1991); Foster, P. & Cairns, J. (1992); Hall, B. (1995c) Genetics of selection-induced mutations. *UvrA*, *UvrB*, *UvrC* and *UvrD* are selection-induced specific mutator loci. *Journal of Molecular Evolution*, 40: 86-93.

<sup>284</sup> Hall, B. (1997) On the specificity of adaptive mutations. *Genetics*, 145: 39-44.

selected become visible/countable, because only those survive and produce clones.<sup>285</sup> That explanation allowed that apparent specificity was created by a more random process of hypermutation. Temperate as ever, Hall argued that specificity was evident in evolving bacterial strains (as the strong models demand), but suggested that a weak interpretation of that appearance was perhaps appropriate.

In 1997, Foster also addressed specificity, but reported that in the F-episome neutral mutations were accumulating alongside adaptive ones.<sup>286</sup> This conclusion was in contrast to her earlier advocacy of mutational specificity. In that paper, Foster corrected her 1994 conclusions<sup>287</sup>; acknowledging that unselected tetracycline mutations in fact arise at roughly the same rate as selected Lac<sup>+</sup> reversions, and that these neutral mutations had been missed in the previous assays because of their extranucleic origin. Foster and Hall's attention to specificity did not lead to the resolution of this contentious issue, although Hall did demonstrate that apparent specificity could be accounted for by the now popular hypermutation models. So in 1997, although the question of specific environment direction had not been resolved, the bacterial phenomenon of adaptive mutation, as it was by this time more commonly termed, was being described as part of a bacteria specific physiological response to stress.

The shift from dual to single aspect in the debate was addressed by Bryn Bridges in a 1997 review article.<sup>288</sup> Through his characterisation of the directed mutation advocates, he reminds us how Cairns' advocacy had always differed from that other supporters. He says that: 'while some evolutionary biologists found the idea of directed mutation disturbing because of its echoes of Lamarckism, molecular biologists displayed their customary ingenuity in proposing ways in which it might be achieved without threatening conventional dogma.'<sup>289</sup> While that characterisation suits Hall, and in some respects Foster, it is much less accurate regarding Cairns. He had not been interested in reducing the impact of directed mutation on the dogma of evolutionary biologists. By the late 1990s, the outcome was that while Cairns had

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<sup>285</sup> The theory of 'selective recapture' was proposed in: Cairns, J. (1995b) Response from Cairns. *Trends in Microbiology*, 3: 293.

<sup>286</sup> Foster, P. (1997)

<sup>287</sup> Foster, P. (1994)

<sup>288</sup> Bridges, B. (1997) Hypermutation under stress. *Nature*, 387: 557-558.

<sup>289</sup> Bridges, B. (1997) p.557



failed to garner support for the strong model with its Lamarckian tones, other directed mutation protagonists had quite successfully promoted the weaker models (i.e. genome-wide hypermutation) and had perpetuated the negotiation of directed mutation in journals in relation to the investigation of those less unorthodox models. Bridges does however confirm that: ‘Although there have been many attempts to prove or disprove the hypothesis of directed mutation, none has been totally convincing.’<sup>290</sup> Bridges’ review in 1997 anticipated the ultimate endurance of both aspects of the debate. He says simply; ‘We shall see.’<sup>291</sup>

And the debate did endure. In April 1997, a team of Estonian microbiologists extended observations of directed mutation to *Pseudomonas sp.*, providing a new prokaryotic example of the phenomenon.<sup>292</sup> At the same time, several researchers began to turn attention to the specific means by which hypermutation, and associated mutator phenotypes, might have arisen in evolutionary history. Because mutation is generally deleterious, evolution has suppressed and minimized the rate of mutation. Hypermutation therefore represents a special case, requiring a new evolutionary explanation for its origin. Lenski re-emerged, co-authoring a paper describing the evolutionary processes underlying the high mutations rates evolved in some pathogens and cancers.<sup>293</sup> Meanwhile, a group of scientists from France, reported that mutator alleles were able to greatly accelerate the mutation rate during directed mutation.<sup>294</sup> Rosenberg confirmed the mutator hypothesis in 1998, demonstrating the existence of a minority of mutator phenotype cells in the bacterial population, capable of transient hypermutation during extreme selection.<sup>295</sup> The physiology of directed mutation was being revealed in more and more detail.

Also, in 1998, members of the core-set revisited the classic lac<sup>+</sup> reversion system for the study of adaptive mutation in stationary phase. Answering critics, Rosenberg, and Foster and Cairns, re-assayed the system to demonstrate that the lac<sup>+</sup>

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<sup>290</sup> Bridges, B. (1997) p.557

<sup>291</sup> Bridges, B. (1997) p.558

<sup>292</sup> Kasak, L., Horak, R. & Kivisaar, M. (1997) Promoter-creating mutations in *Pseudomonas putida*: a model system for the study of mutation in starving bacteria. *Proc. Natl. Acad. Sci., USA*, 94: 3134-3139.

<sup>293</sup> Sniegowski, P., Gerrish, P. & Lenski, R. (1997) Evolution of high mutation rates in experimental populations of *E. coli*. *Nature*, 387: 703-705.

<sup>294</sup> Taddei, F., Maynard-Smith, J. *et al* (1997) Role of mutator alleles in adaptive evolution. *Nature*, 387: 700-702.

<sup>295</sup> Rosenberg, S., Thulin, C. & Harris, R. (1998) Transient and heritable mutators in adaptive evolution in the lab and in nature. *Genetics*, 148: 1559-1566.

revertants (lactose digesting bacteria) emerged post-selection for lactose digestion and were thereby adaptive in their nature, rather than random spontaneous pre-selection mutants.<sup>296</sup> This re-asserted the anomaly that formed the basis of the molecular aspect of the debate, and highlighted that 10 years of negotiation had still not resolved the central issue. Also, Hall provided a review article summarising the data since 1993, arguing for the strong interpretation of 'direction' that: 'Adaptive mutations are spontaneous mutations that occur in microorganisms during periods of prolonged stress in non-dividing or very slowly dividing populations...that are specific to the environmental challenge that causes that stress.'<sup>297</sup>

In 1998/1999 the first decade of directed mutation was evaluated in several review articles. Patricia Foster summarised the findings from the first ten years, describing how it had come to be understood that: '...[Although] the mutations that arise during selection are not 'adaptive' in the original sense, the mutagenic mechanism that produces these mutations may nonetheless be of evolutionary significance.'<sup>298</sup> Foster cited the various hypermutation hypotheses (hypermutation in a sub-population, transient hypermutation, hypermutation of certain gene loci, or hypermutation of the entire genome) as the likely mechanisms of adaptive mutation. She did not conclude on the specificity of the mutations, nor did she expand upon what the 'evolutionary significance' of the mutations might be.

In the same volume of *Genetics* Rosenberg provided a similar review of the first decade of research.<sup>299</sup> In that paper, she and her co-authors identified 1997 as a turning point in the debate. They suggested that at that time attention shifted towards hypermutation models as the principle of directed mutation. Both Foster and Rosenberg's reviews include details of new hypotheses for the hypermutation mechanism of directed mutation; even as they reviewed the debate, they contributed to its next phase. They remained firmly focussed on the molecular biological debate as they instigated that next phase, with both new hypotheses being technical and largely specific to molecular biology. To clarify that focus, Rosenberg added in her

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<sup>296</sup> Foster, P. & Cairns, J. (1998) Adaptive mutation of a lacZ amber allele. *Genetics*, 150: 1329-1330;  
McKenzie, G. Lombardo, M. & Rosenberg, S. (1998) Recombination-dependent mutation in *Escherichia coli* occurs in stationary phase. *Genetics*, 149: 1163-1165.

<sup>297</sup> Hall, B. (1998a) Adaptive mutagenesis: a process that generates almost exclusively beneficial mutations. *Genetica*, 102/103: 109-125.

<sup>298</sup> Foster, P. (1998) Adaptive mutation: has the unicorn landed. *Genetics*, 148: 1453-1459.

<sup>299</sup> Rosenberg, S., Thulin, C. & Harris, R. (1998)

conclusion that, in the past year, the mutational process under study had been shown to be not 'directed in a Lamarckian way to selected genes'.<sup>300</sup>

A review by John Cairns appeared alongside Foster and Rosenberg's papers, in which he described the relationship between cancer studies and the study of directed mutation phenomena.<sup>301</sup> In addition, both Foster and Rosenberg also offered further reviews of the first decade of directed mutation elsewhere. In a later contribution to *Genetics*, Rosenberg summarised the facts that had been determined in relation to directed mutation.<sup>302</sup> Her team stated that adaptive mutation is recombination dependent, occurs only during stationary phase and only under conditions of non-lethal selection. They concluded that its most likely cause was recombination-dependent stationary phase hypermutation. A few months later Foster echoed their conclusions in a contribution to *The Annual Review of Genetics*.<sup>303</sup>

Meanwhile, Hall pursued his study of the *ebg* gene. In 1998, he published results showing that mutation in that gene was regulated by activity at another gene locus (PhoPQ).<sup>304</sup> Disruption of the PhoPQ gene dramatically reduced the rate of adaptive mutation in the *ebg* gene. The fact that mutation at *ebg* was being regulated by another gene implied that mutation at that site was not simply the result of a stress response based on slow repair or damage. Rather, gene control implied an evolved system for achieving adaptive mutation at *ebg*. Hall's observations indicated that adaptive mutation at *ebg* at least was not a result of starvation stress, but rather a response to it. Interestingly, having been the least inflammatory of the advocates in the first few years of the debate, by 1997 Hall was focussing more attention on the contentious issue of specificity than any other researcher.

By the late 1990s, at a superficial level, the argument concerning the role of strong versus weak models of directed mutation appeared to have been resolved by the explanations coming from the hypermutation advocates. For the molecular biologists, directed mutation could be expressed as a special feature of extra-nuclear

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<sup>300</sup> Rosenberg, S., Thulin, C. & Harris, R. (1998) p.1564

<sup>301</sup> Cairns, J. (1998)

<sup>302</sup> McKenzie, G., Lombardo, M. & Rosenberg, S. (1998).

<sup>303</sup> Foster, P. (1999) Mechanisms of stationary phase mutation: a decade of adaptive mutation. *Annual Review of Genetics*, 33: 57-88.

<sup>304</sup> Hall, B. (1998b) Adaptive mutagenesis at *ebgR* is regulated by PhoPQ. *Journal of Bacteriology*, 180: 2862-2865.

evolution in stressed bacterial populations. And for the evolutionary biologists that sufficed as resolution. Provided that the problem of directed mutation had an explanation within molecular biology the conflict was under control. Without the implications of the strong models, directed mutation no longer provided the material of an assault on the authority of evolutionary biology. In that regard, in the journal based phase of the debate, the boundary work of directed mutation had failed to extend the authority of molecular biology into the territory of evolutionary biology.

In terms of the journal-based debate, the strong Cairnsian version of directed mutation, which had always been the most controversial element of the debate, had been eclipsed by the discussion of weaker interpretations. The aspect of the debate that dealt with the resurrection of Lamarckism had been overshadowed by technical investigations and negotiations. In 1998, immunologist, and Lamarckian enthusiast, Edward Steele ascribed the absence of Lamarckian debates in the key journals to a conspiracy amongst journal publishers to suppress anti-Darwinian investigations.<sup>305</sup> In the case of directed mutation, the eclipse of the Lamarckian elements in the journal debate appears to have less sensational causes.

First, the requirement asserted by the critics, and perceived by the advocates, that a mechanism be determined had not been achievable in relation to the strong models. This was recognised by several of the advocates by the mid 1990s, notably Patricia Foster, and they began to pursue models of directed mutation that might yield mechanism. By moderating their approach regarding the significance of directed mutation for evolutionary theory, those advocates enabled the debate to continue in a form that journal publishers could entertain i.e. as a debate that seemed to produce new data and valid points for negotiation. Second, even beyond the problems of finding evidence for the strong models the advocates had begun to perceive the huge burden of proof that rested on their community whilst directed mutation existed as a challenge to evolutionary theory. Several authors had begun to think of Darwinism as defended as a kind of religious principle, and perhaps retreated from the strong models as a reaction to the degree of dissent that they had precipitated. The advocates' approach after 1994 stripped away some of that burden, and allowed the molecular biological debate to proceed. Thirdly, it had chiefly been Cairns who had

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<sup>305</sup> Steele, E., Lindley, R. & Blanden, R. (1998)

advocated the strong models; he had launched the authority bid based on those models and had managed the dissent that was precipitated (answering Lenski and Mittler for example on several occasions and engaging in personal exchanges with many critics). As retirement distanced him from the debate, and his former graduate student (Foster) took up his mantle with a more moderate twist, the strong models necessarily had a decreased presence.<sup>306</sup>

The circularity of the directed mutation debate continued to be an impediment to resolution. While Foster and Rosenberg<sup>307</sup> took on a more temperate attitude to the contentious issues of ‘environment direction’ and specificity, others began to revisit the stronger versions of adaptive mutation. In 2004, Foster remarked that ‘it is interesting that the original Roth-Stahl model has evolved so that it is now almost exactly what Cairns and I proposed years ago.’<sup>308</sup> Meanwhile Foster stated, in her own view, that mutations were not directed in the way that she and Cairns had first thought, but that rather adaptive mutation was a complicated stress induced response, that depended on selection, but was not directed by it as such.<sup>309</sup> With all this circular argument the strong version of directed mutation, the Lamarckism versus Darwinism

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<sup>306</sup> As late as 2003 Cairns and Foster spoke out to support the ‘transient hypermutating sub-population theory’ that Hall (1990) had previously championed. In that paper, Cairns and Foster still referred to ‘the evident ability of *E. coli* to produce plenty of Lac<sup>+</sup> revertants when they are needed’, and added the ‘stronger’ question of whether ‘...the existence of hypermutators [is] simply the happy result of other reasons for selecting that degree of precision of gene expression’. Cairns, J. & Foster, P. (2003) The risk of lethals for hypermutating bacteria in stationary phase. *Genetics*, 165; 2317-2318. Also, Foster continued to advocate hypermutation elsewhere: Foster, P. *et al* (2003) Error prone polymerase, DNA polymerase IV, is responsible for transient hypermutation during adaptive evolution in *Escherichia coli*. *Journal of Bacteriology*, 185(11): 3469-3472.

<sup>307</sup> By 2000, both Foster and Rosenberg were exploring the specifics of hypermutation as an agent of adaptive mutation, and both eventually concluded that a Lamarckian type mechanism was probably not at work. See: Bull, H., McKenzie, G., Hastings, P. & Rosenberg, S. (2000a) Evidence that stationary-phase hypermutation in *Escherichia coli* is promoted by recombination. *Genetics*, 154: 1427-1437; Bull, H., McKenzie, G., Hastings, P. & Rosenberg, S. (2000b) The contribution of transiently hypermutable cells to mutation in stationary phase. *Genetics*, 156: 925-926. Foster, P. (2003); Foster, P. (2004) Adaptive mutation in *Escherichia coli*. *Journal of Bacteriology*, 186: 4846-4852.

<sup>308</sup> Foster, P. (2004). Stahl and Roth had both previously offered ‘weak’ models of directed mutation that allowed for a fully Darwinian interpretation, but by the late 1990s had strengthened the models and focussed on the gene amplification theory. See: Andersson, D. Slechta, S. & Roth, J. (1998) Evidence that gene amplification underlies adaptive mutability of the bacterial *lac* operon. *Science*, 282: 1133.

<sup>309</sup> Foster, P. (2004)

debate, and the authority challenge that all this implied, might have reached a form of natural closure.<sup>310</sup> And yet it did not.

### **2.3 The extension of the directed mutation debate in the Internet context: changing scale and agendas as a new community takes on the controversy.**

As hypermutation became the primary journal-based theory for the mechanism of directed mutation, Cairns' original strong theory found support in a far more prolific medium. In the late 1990s the Internet was emerging as a new forum for debate on the Lamarckian implications of directed evolution, and with this transition came a vast array of new participants. What had previously been a controversy significant to a handful of researchers, constituting a core-set, was becoming the intellectual property of a fascinated lay public and a broader scientific community.

Pre-existing material from the journal-based debate was prolifically reproduced in this new medium, alongside novel discussion material of this unexplained bacterial phenomenon. The journal materials were re-evaluated and re-expressed in the rhetoric of many and diverse agendas. As the theory of directed mutation was transformed by extended participation, it was re-shaped to become a conceptual modelling tool for mathematics and computer science, an example of the repression of unorthodoxy in science, and even a tenet of a broader spiritual appreciation of self will and progress. The fact that the Internet provided this opportunity for communication between individuals from effectively different 'social worlds', makes it a candidate for description as a 'boundary object'.<sup>311</sup> As a 'boundary object' the Internet allowed communication across the boundaries between scientists, amateurs scientists and the public. It allowed the material of the directed mutation debate to be translated into forms comprehensible to the different groups, but that was not under the control of any of those groups. Although it is perhaps

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<sup>310</sup> The modes of closure in scientific debates are discussed in Chapter 1. Closure by 'abandonment', or by apparent resolution, are just two of a range of mechanisms by which debates are considered to reach resolution. See Ed. Engelhardt & Caplan (1987)

<sup>311</sup> The notion of 'boundary objects' has been introduced in: Star, S & Griesemer, J. (1989) Institutional ecology, 'translations' and boundary objects: amateurs and professionals in Berkley's museum of vertebrate zoology, 1907-39. *Social Studies of Science*, 19: 387-420.

analytically interesting I will not pursue this interpretation of the Internet, since I am more interested in the notion of the Internet forum as a physical arena for debate.

It is at the stage of uptake of the directed mutation debate by the online community that the core-set terminology, useful to the description of the debate in the professional journal context, loses its descriptive power. Extension in the Internet forum increased the scale of the debate manifold beyond Collins' allowances, and makes his terminology less suited to description of directed mutation online.<sup>312</sup> Although some of the original core-set remain engaged with the debate, they are now part of a much larger and more diverse community. That larger community has multiple agendas and is comprised of specialists and non-specialists, leaving it outside the descriptive power of Collins' model.

In the Internet forum, the scientific debate continued. In the personal pages of academics, or on professional and institutional sites methodology and mechanism were discussed. These materials existed alongside amateur discussions and new interpretations of the journal debate. In this section, I introduce some of the online materials, to illustrate the changing identity of the debate in this forum. The structure and dynamics of the debate online are considered in detail in Chapter 5, where the growth and change of the debate in the Internet forum is addressed explicitly and examined empirically.

The appeal of directed mutation in the Internet forum was perhaps enhanced by the broader cultural context of the 1990s. In that period, the public understanding of science had become a key issue, and there was much debate about how science should be communicated.<sup>313</sup> Popular interest in science was increasing, fostered through mass media attention to the public understanding of the new sciences of reproductive technology, cloning and the genetic modification of foodstuffs.<sup>314</sup> To the

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<sup>312</sup> The failure of Collins' core-set terminology to describe the debating community after uptake on the Internet need not indicate that its use as a grouping tool for the early debate is undermined. Collins acknowledges that the core-set rarely persists beyond a ten-year period [Collins, H. (1981)]. He attributes this decay to the usual processes of controversy resolution, or to the abandonment of the controversy. In the case of directed mutation, it seems that the change of scale diffused the core-set, and that this could be acknowledged as an additional means by which a core-set may decay in a natural way.

<sup>313</sup> Gregory, J. & Miller, S. (1998) *Science in public*. Perseus Publishing, Cambridge, MA; Lock, S. (2008) *Lost in translations: discourse, boundaries and legitimacy in the public understanding of science in the UK*. PhD Thesis, University College London.

<sup>314</sup> Levy, J. (2004) *Controlling the course of scientific advance: the case of human embryology*. PhD thesis, University of Southampton.

newly engaged public some of the most appealing aspects of science were the controversies and moral debates. In particular, unorthodox science of the type that fringe journals such as the *Fortean Times* have documented captured the public imagination, and these kinds of publication began to serve a growing public interest. Lyall Watson's 1991 Foreword to *The Best of the Fortean Times* says of the procedures of establishment science and mainstream science publication that:

‘ Most workers accept that these procedures are cumbersome and make science inherently conservative and resistant to change. But what is not generally acknowledged is that this is a political process rather than a scientific one. It depends upon personal preference, upon the votes of a scientific jury - every member of which would be disqualified from any normal inquiry on the basis of blatant conflict of interest. And yet it is on the verdict handed down by such courts that we are expected to exercise our preferences and construct our beliefs about the world. And that just isn't good enough.’<sup>315</sup>

Much of the late 1990s Internet material on directed mutation served this type of agenda, and the cultural enthusiasm for controversy and unorthodoxy made the story of directed mutation popular.<sup>316</sup> In the context of the Internet forum, the directed mutation debate was easily translated to become a narrative of unorthodox research, potentially threatening to the mainstream of science and significant to our view of human evolution. In that construction the research was characterised as unfairly marginalized, and discredited through a broader establishment conspiracy to suppress controversial or unorthodox findings. That interpretation was encouraged by some of

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<sup>315</sup> Watson, L. (1991) Foreword. In: A. Sisman (Ed.) *The best of the Fortean Times: the journal of strange phenomena*. Futura Publications, London. p.7

<sup>316</sup> This broad enthusiasm for anomalous/unusual science in the late century is even reflected in the pages of the prestigious journal *Nature*. In 1988-1989 the journal hosted the debates on ‘the memory of water’ (1988), ‘directed mutation’ (1988-1989) and ‘cold fusion’ (1989). Editor John Maddox presented each of these cases alongside editorial reservations, which appealed for reader's scepticism. See: (on water memory) Maddox, J. (1988a) Editor's note: When to believe the unbelievable. *Nature*, 333: 787; (on directed mutation) Stahl, F. (1988); (on cold fusion) Maddox, J. (1989b) Another red herring leads nowhere. *Nature*, 339: 253. Maddox presented this contentious material to a contemporary readership with an enthusiasm for such dissent, but did so without compromising the legitimacy of the journal or his editorial process. Thus, he achieved a compromise between the readership's wants and the objectives of the journal.



early journal publications, in which the defence of Neo-Darwinian orthodoxy inspired some advocates of directed mutation to complain of unfair treatment, and some commentators to acknowledge the special burden of proof facing non-Darwinian theorists.<sup>317</sup>

The cultural enthusiasm for this construction of the repressive nature of orthodox science was enhanced by the longer-term growth of public distrust of science. Throughout the second half of the twentieth century the public perception of science had changed. The scientist was no longer viewed as the white haired genius, questing for knowledge through a benign tradition. Rather, science and the scientist had been transformed in popular perception into a servant of the state, bound by funding to undertake potentially corrupt and ethically flawed research.<sup>318</sup> This loss of faith in the objectivity and honesty of the scientific profession had begun in the post-war climate, in which as sociologist Jerome Ravetz acknowledges: 'it was in the public record that with the A-bomb science had tasted sin, and that with the H-bomb it had found it sweet'.<sup>319</sup> This sense of corruption was fed in the 1990s as tales of unlicensed human cloning experiments and illegal assisted reproduction procedures filled the popular press.<sup>320</sup> The resulting public perception of science in the late 1990s, as the directed mutation debate moved into the public arena through its extension in the Internet forum, was ripe to accept this debate as an example of repression of unorthodoxy by a mainstream of corrupt self-serving science professionals. In this light, directed mutation had been discredited to protect established careers and research grants of those individuals who held authority and wielded it to their advantage.

There are several instances of the uptake of directed mutation to suit this type of agenda. William Corliss' site 'Science Frontiers' is perhaps the most illustrative example. This is an on-line bi-monthly newsletter that reviews the 'unusual and unexplained' drawn from the world's journals. Corliss describes the features he selects as: 'those observations and facts that challenge prevailing scientific

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<sup>317</sup> See for examples: Cairns, J. (1993); Foster, P. (1993)

<sup>318</sup> Levy, J. (2004)

<sup>319</sup> Ravetz, J. (1990) Orthodoxies, critiques and alternatives. In: R. Olby, G. Cantor, J. Christie & M. Hodge (Eds.) *Companion to the history of modern science*. Routledge, London. p. 902

<sup>320</sup> Levy, J. (2004)

paradigms'.<sup>321</sup> The Science Frontiers newsletter first referenced the directed mutation research in 1988, directly after the publication of Cairns, Overbaugh & Miller (1988). Corliss describes the research findings and asks: 'Could anything be more heretical?'<sup>322</sup>, he adds: 'Will *Nature* now dispatch a 'hit squad' to Harvard?'<sup>323</sup> Again in 1989, 1990, 1991 and 1994 Corliss' newsletter discussed directed mutation. The 1990 November- December issue of the newsletter demonstrates some of the sensationalism which often characterises the construction of science to suit this kind of agenda. Concerning the concept of mutation under stress he asks: 'What sort of environmental stress would cause humans to mutate? What would we turn into if, say, global temperatures rose 5 degrees?'<sup>324</sup> In the 1991 May-June issue Corliss suggests that the Darwinism versus Lamarckism aspect of the debate has been at the heart of the 'discredit' of Cairns' research.<sup>325</sup> In the 1994 November- December issue Corliss reiterates his conclusion concerning Cairns' research that: 'This claim was too awful to accept'.<sup>326</sup>

Corliss' site is important in several respects. While it gives attention to a vast range of subjects generally considered as pseudo-science, it is also replete with intelligent comment on issues drawn from scientific literature, such as directed mutation. Corliss demonstrates an understanding of the debates that he addresses. He is trained as a scientist, having a bachelor and masters degree in physics. Corliss had been producing his newsletter commentaries since early on in the journal-based phase of the debate, but his independent status and position outside an academic institution rendered him part of the crowd of onlookers that surround the core-set.

As the journal debate moves away from Lamarckian references, and towards the weaker hypermutation models, Corliss' material becomes more significant. As the Lamarckism versus Darwinism aspect of the negotiation was eclipsed in the journals,

<sup>321</sup> [www.science-frontiers.com](http://www.science-frontiers.com)

<sup>322</sup> [www.science-frontiers.com/sf060/sf060p07.htm](http://www.science-frontiers.com/sf060/sf060p07.htm)

<sup>323</sup> [www.science-frontiers.com/sf060/sf060p07.htm](http://www.science-frontiers.com/sf060/sf060p07.htm) Here, Corliss is alluding to the treatment of scientist Jacques Benveniste in the period after his controversial 'water memory' findings were published in the journal *Nature* in the late 1980s. Having published his work, the editorial team then dispatched a group to Benveniste's laboratory to test the validity of his results. They concluded from this highly unusual visit that his results were invalid, and thus the same journal that had validated his work with publication shortly afterwards reported the disproof of his findings. For details of the water memory case see Chapter 5.6

<sup>324</sup> [www.science-frontiers.com/sf072/sf072b06/htm](http://www.science-frontiers.com/sf072/sf072b06/htm)

<sup>325</sup> [www.science-frontiers.com/sf075/sf075b06.htm](http://www.science-frontiers.com/sf075/sf075b06.htm)

<sup>326</sup> [www.science-frontiers.com/sf096/sf096b08.htm](http://www.science-frontiers.com/sf096/sf096b08.htm)

sites like Corliss' became the refuge for that part of the debate. I have already argued that the molecular biological problem of directed mutation could not be resolved without resolution of the Darwinism versus Lamarckism element of the debate. So while the journals hosted the negotiation of the molecular biological aspect of the debate, the Internet became the forum for negotiation of the broader contest.

In Internet keyword searches for directed mutation, 'Science-Frontiers' always features in the top ten of the hits generated. This is one of the key criteria for how visible an Internet resource will be, since people tend to view only the first few hits generated by any particular search. In addition, other sites that focus on fringe science phenomena frequently cite or link to Corliss' material on directed mutation.<sup>327</sup> This further raises the profile of the site. Corliss presented a new construction of the directed mutation debate to an interested popular audience, while retaining a link to the journal debate through citation and the reproduction of paper sources online. Science-Frontiers provides an example of some of the contributions that bridged the gap between the professional and popular during uptake in the Internet forum.

Internet sites like Corliss' retain a link to the scientific debate in as much as they refer to published material, engage with the scientific details with some degree of understanding and interpretative ability, re-print articles and revisit the debate over a period of time adding further commentary when new resources arise in the print medium. By contrast, there are numerous sites that take only the most basic principles of directed mutation and put these to work within their own agenda, without further recourse to scientific reference and in the absence of skills for the evaluation of the journal materials. Various sites of this quality exist on the Internet, with perhaps the most significant being related to the discussion of science, religion and spirituality issues.

An example of this form of uptake exists in Henry Bayman's online book '*Science, Knowledge and Sufism*'.<sup>328</sup> Bayman cites Cairns' work on directed mutation as an example of how science has shown that starvation conditions lead to genetic behaviours outside the explanatory power of standard scientific theory. He suggests that this provides scientific proof that hypo-nutrition or ascetic starvation, as carried

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<sup>327</sup> See for example: [www.talk-origins.com](http://www.talk-origins.com) & [www.amasci.com](http://www.amasci.com)

<sup>328</sup> Bayman, H. (2000) <http://home.att.net/~nungan/sufiway> [offline Dec. 2004]

out in some religions (i.e. Buddhism), can lead to the altered states of consciousness which these groups claim some of their members can achieve. He states that directed mutation is evidence that the human spirit, when subjected to starvation conditions, can achieve changes outside the laws of physics, just as the bacteria can achieve mutations which fall outside the explanatory power of molecular genetics. Bayman overlooks that, in the bacteria, mutation is directed to the achievement of substrate utilisation for the *avoidance* of starvation. He also fails to appreciate that the bacteria have only been starved of certain nutrients (he says they have been given 'no food'). Although commentary of this kind does not contribute explicitly to negotiation of either aspect of the directed mutation debate, it does promote further engagement. In that regard it serves to help increase the life expectancy of the debate, bringing new individuals into contact with the concepts and diversifying the meanings of directed mutation. Furthermore, Bayman's knowledge of this subject is possibly an example of the way in which directed mutation was being disseminated across the Internet. Although Bayman may not have come into contact with the theory in online materials, it seems a more likely point of contact than the scientific literature.<sup>329</sup> At the very least, Bayman's discussion is an example of the uptake of a controversial scientific principle, in a form far removed from the construction that exists in paper publication, to suit a substantially different agenda.

As the meanings of directed mutation diversified in the Internet forum two new scientific applications for the theory arose: i) directed mutation was incorporated as an algorithm in evolutionary programming in computer science and ii) directed mutation was described as a factor pertaining to quantum theory. This uptake and use of the theory illustrates how the Internet allowed directed mutation to take on new meanings and be absorbed into other disciplines. In these two examples, directed mutation was transferred into other professional contexts.

The computer science of 'evolutionary computing' was developed during the 1960s, as a tool for the potential evolution of programming sequences of artificial

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<sup>329</sup> Bayman's brevity of explanation and almost total lack of provision of scientific information pertaining to the phenomenon of directed mutation make it unlikely that he synthesised his treatment from journal sources. There are no references for the material he mentions, and not even a full name for Cairns. This indicates that Bayman's information has probably been constructed from one or a few poorly detailed secondary sources online. What Bayman offers is probably a third hand account of directed mutation.

intelligence (AI) systems.<sup>330</sup> This programming assay utilised the population, mutation and evolution concepts drawn from biological evolutionary theory as a problem-solving device by which to select effective programming sequences for the design of AI computer systems. The theory for this system is premised on the mapping of multiple solutions (appearing as possible computer code sequences) to any given problem within a 'search space', areas of which have higher fitness than others, measured by the usefulness of the solutions in that particular cluster area. Populations can be formed on the basis of selection for highest fitness of the solution to the given problem, and from these populations new generations (of computer sequence) can be bred through cross over and mutation. The extent of cross over and mutation can be set by the programme's operator, such that various rates and extents of change over time (evolution) can be assayed.

The more sophisticated system of genetic algorithms was proposed in 1975 by John Holland, who outlined a form of evolutionary computing premised on the precise laws of chromosome, gene and nucleotide action as expressed in the biological theory of genome evolution.<sup>331</sup> In 1992, a workable system of 'genetic programming' was created by John Koza, in which sequence evolution could be assayed through the rules of molecular genetics.<sup>332</sup> In the late 1990s, the concept of directed mutation came into use as one of the test algorithm tools applied by evolutionary genetics programmers. The element of direction, when applied through evolutionary computing, determined that the mutation phase of the assay would occur inline with some pre-determined rules, rather than at random as usual. These rules allow new sequences to be generated with a tendency towards the incorporation of sequence elements that have been useful products of past assays. Therefore, the kinds of mutation that are generated have a view to utility; they are directed to be useful by the criteria that have been developed in past assays. The rise of this type of evolutionary computing in the late 1990s illustrates the diffusion of meanings of directed mutation that occurred in the Internet forum. It demonstrates how the theory was taken up and modified to suit a different agenda or purpose. Computer

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<sup>330</sup> The invention of evolutionary computing is credited to Rechenberg, who detailed the original full system in: Rechenberg, I. (1973) *Evolutionsstrategie*. Formmann-Holzboog, Verlag, Stuttgart.

<sup>331</sup> Holland, J. (1975) *Adaptation in natural and artificial systems*. University of Michigan, Michigan.

<sup>332</sup> Koza, J. (1992) *Genetic programming: on the programming of computers by means of natural selection*. MIT Press, Cambridge, Massachusetts.

programmers were not interested in the authority dispute underlying the directed mutation debate, in fact, they were not even interested in the legitimacy of the directed mutation claims. Rather, the notion of directed mutation was taken up in abstraction from those negotiations, and put to work in principle.

Recently there has been an attempt to apply the theory of quantum mechanics to evolutionary theory. In 2000 Johnjoe McFadden published a theoretical text on this connection entitled *Quantum Evolution*.<sup>333</sup> In that text, McFadden presented a bold hypothesis, predicting that, since the material of heredity is located in the single molecule DNA, the processes involving that molecule will be governed by quantum laws. He describes how adherence to these laws would confer upon organisms the ability to control specific molecular mechanisms, including for example the capacity to mutate their genes specifically and as a response to environmental pressures. McFadden claimed this capacity could represent the root of consciousness and free will. The claims of this text, although profoundly speculative, illustrate a meeting of various incarnations of directed mutation. The text uses concepts drawn from the computer modelling of genetic and evolutionary phenomena (genetic algorithms), evidence from the molecular study of the behaviour in bacterial genomes under stress (the original directed mutation research) and the concepts of the origin and function of free will and spirituality (as linked to directed mutation by some spiritualist groups publishing online). McFadden's intellectual project is illustrative of the state of the directed mutation debate in the period after its extension of scale in the Internet forum. It is conceivable that his hypothesis could not have come about without the changed scale of the debate in the late 1990s.

In terms of the Internet forum providing an extension of the journal-based debate, the contributions have been mixed. Some contributions remain professional, and some professional materials are reproduced and made available online. Also, some contributions bridge the gap between the professional and popular, referring to and reproducing professional materials, and offering non-professional commentary on them. Some of the other materials are more plainly non-professional, and are completely discrete from the journal-based debate. In other cases directed mutation has been taken up in abstraction from the debate and been put to 'in principle' use.

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<sup>333</sup> McFadden, J. (2000) *Quantum evolution: life in the multiverse*. Penguin, London.

The relative proportions of each of these kinds of treatment are described in Chapter 5, in which a quantitative analysis illustrates their relative scale change.

Overall, the focus of the online materials is more on the Darwinism versus Lamarckism aspect of the debate than the molecular biological aspect. The Lamarckism versus Darwinism dissent is approached in this forum with renewed vigour, and without the constraints of journal editors and peer review that material is able to come to the fore. The situation at the time of writing is that the journal debate provides the forum for the negotiation of the molecular biological phenomenon of directed mutation in unicells. In that forum the Darwinism versus Lamarckism problem is still acknowledged, but has not commonly been negotiated explicitly since the mid 1990s. Meanwhile, the Internet provides the forum for the negotiation of the more contentious material of the Lamarckism versus Darwinism aspect of the debate. The molecular biological problem is also negotiated online, but often only in treatments motivated by attention to the broader contest.

The result is that the Internet provides the context for an ongoing challenge to the authority of evolutionary biology. In the Internet forum that challenge has been taken up from a range of perspectives. Some contend that evolutionary biology has assumed authority that should be allocated to creation science. Others challenge the authority of the discipline by asking what justifies the monopoly of Darwinism. Though these challenges take a different form from Cairns' in 1988, the result is similar in that the authority and boundaries of evolutionary biology are being questioned and challenged. So, as the boundary contest is eclipsed in the journal treatments with the decline of attention to the broader debate, it becomes apparent that we must follow that level of dissent into the new arena of the Internet forum. Chapter 5 seeks to achieve that tracking of the two aspects of the debate.

This chapter highlights key features from the history of the directed mutation controversy. From this narrative the areas of interest become apparent, and the two aspect nature of the debate is elucidated. The issues arising from this narrative are explored further in the following chapters. In Chapter 3, the Lamarckism versus Darwinism aspect of the debate is examined, and the historical legacies underlying that conflict are discussed. In that chapter this case study is located within the context

of the broader and more enduring debate. In the language of cultural cartography I identify 'Lamarckism' and 'Darwinism' as 'old maps', and the activity of the directed mutation debate is located in relation to those. Through this discussion the significance of Lamarckian association in the late twentieth century is clarified. In Chapter 4, some of the intrinsic factors of both aspects of the debate are examined. The clash of scientific disciplines and Cairns' particular style of advocacy are explored, and each of those features is considered as an agent of controversy perpetuation. I frame the debate as a boundary contest, and consider the activity/motivation of advocates and adversaries in that light. In Chapter 5, the impact of uptake of the debate in the Internet forum is examined empirically, and a detailed analysis is provided of the scale and identity change of the debate in that context.



### **Chapter 3: The intellectual and cultural context of the directed mutation debate: an agent of controversy perpetuation?**

In this chapter I discuss three of the six perpetuating forces that I identify in this project: historical legacies, the use of ‘old maps’ of cultural authority, and scientific dogma. I suggest that the context created by these factors resulted in the protraction of the directed mutation debate. I argue that the directed mutation debate has not been ‘local’ or ‘episodic’, but instead is part of a larger ongoing conflict in evolutionary biology, and that, action and motivation in the contest have been determined by the legacies of the larger conflict. This chapter places directed mutation in the intellectual and cultural context of late twentieth century evolutionary debate. In particular, I argue that widespread cultural and intellectual adherence to Darwinism can be identified as a key force in the generation of negative responses to the directed mutation research (section 3.1). Also, I argue that antagonism towards directed mutation was reinforced by contemporary negative perceptions of both Lamarckian theory and the previous attempts to reinvent Lamarckism (section 3.2). I suggest that ‘old maps’ of Darwinism and Lamarckism were consulted during the directed mutation debate, thus bringing history to bear on that contest. Finally, I describe certain dogmas that arose in evolutionary biology as a result of the historical legacies of Darwinism and Lamarckism. I argue that these dogmas reinforced the details of the old map, and acted as impediments to effective negotiation (section 3.2.3).

In section 3.1, I argue that the legacy of the rise of Darwinism during the twentieth century significantly influenced negotiation in the directed mutation debate. I illustrate that widespread intellectual and cultural attachment to Darwinian theory was a feature of the late twentieth century. I use Thomas Gieryn’s concept of ‘old maps’<sup>334</sup> to describe how a stable Darwinian authority was constructed during the twentieth century and subsequently deployed in new conflicts pertaining to evolutionary theory. I argue that the authority, or even monopoly, of Darwinism was recorded on an old map, ready to be unfurled in new contests as a tool for the protection of existing authority.

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<sup>334</sup> Gieryn identifies the possible role of old maps in Gieryn, T. (1999a). The concept of old maps, and their relevance to this project, is discussed in Chapter 1 (section 1.3).

I use the language of boundary work to describe how individuals throughout the twentieth century contributed to the construction of the old map and the enduring identity of Darwinism that it represents. In particular, I describe the activities of the architects of the modern synthesis as boundary work for monopolization, and demonstrate how they were able to vastly increase the authority attached to Darwinian theory. I argue that the Darwin centennial in 1959 can be identified as the point at which the constructed authority of Darwinism began to be deployed as an ‘old map’ and to persist between conflict episodes. I argue that reference to the old map of Darwinism during the directed mutation debate complicated the conflict, and increased attention to the implications of the molecular phenomenon for evolutionary theory in general.

In section 3.2, I argue that an old map record of Lamarckism was also developed during the twentieth century, and later called upon in the directed mutation debate. The old map record of Darwinism confers authority, acting as a summary of all the authority won during a series of conflict episodes. Conversely, the Lamarckian old map reinforces the lack of authority attached to Lamarckism. I describe how the progressive defamation of Lamarckism, coupled with the rise of Darwinism, reinforced a persistent negative identity of Lamarckian theory. I argue that the defamation of Lamarckism became a tacit understanding, which ultimately influenced the directed mutation debate. I show that reference to the old map of Lamarckism itself became contentious. I describe how the old map identity of Lamarckism was constructed from the sum of failed attempts by Lamarckians to gain authority. In particular, I discuss how the map records the contributions of Paul Kammerer and Trofim Lysenko. I describe the historical construction of those individuals as what I term ‘iconic failures’, and show their role as a foil to the Darwinian icon that was created during the centennial celebration.

In section 3.3 I discuss the impact of scientific dogma on the directed mutation debate. I describe how the old map has been reinforced by a series of persistent dogmas (or laws) in evolutionary biology. In this section, the construction of dogmas by Darwinians is described as one element of their boundary work for monopolization. In particular, I describe Francis Crick’s ‘central dogma of evolutionary biology’ and argue that this represents a key landmark on the old map

of Darwinism. I suggest that the dogmatism of evolutionary biology generated a particularly unreceptive climate for the directed mutation research. I argue that the requirement that directed mutation theorists overcome dogma forced increased obligations of proof and refutation upon them.

Overall, this chapter describes how factors beyond the local and episodic have contributed to the negotiation of directed mutation. The genesis, perpetuation and deployment of certain pro-Darwinian and anti-Lamarckian attitudes are analysed here to illuminate the context for non-Darwinian work in the late twentieth century. This chapter demonstrates how some well-developed preconceptions of both Darwinism and Lamarckism can be viewed as determinants of the quality and protraction of the directed mutation debate. I aim to illustrate that with this scientific controversy, and perhaps scientific controversies more broadly, the quality and dynamics of the debate do not rest only on the participants' engagement with the specific material of the contentious research. Rather, engagement is influenced more broadly by the social and intellectual context, and is informed or enforced by reference to old maps, endowing certain biases and predilections that underscore the activity of advocates and adversaries. Recognition of the effects of such legacies is essential to understanding the tone and protraction of the directed mutation controversy.<sup>335</sup>

### **3.1: A context for Darwinian adherence in the late twentieth century: the creation of an old map of Darwinism.**

Chapter 1 identified the two aspects of the directed mutation controversy. The first comprising methodological and theoretical issues, the second concerning the broader conflict of Darwinism versus Lamarckism. That second aspect involves attempts to maintain the authority of Darwinism as the principal theory of evolution versus the challenge from individuals who seek to garner some of that authority for

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<sup>335</sup> Phase 3 of Harry Collins' empirical program of relativism (EPOR) [see Collins, H. (1981a) Stages in the empirical programme of relativism. *Social studies of Science*, 11: 3-9] seeks to identify the influence of social and cultural factors on the negotiation of dissent and the achievement of consensus. Trevor Pinch has pointed out that few analyses have achieved phase three of the program. This project aims to address negotiation in the directed mutation controversy in relation to the influence of history and cultural context, and thereby achieve analysis that reaches phase 3 of the EPOR.

non-Darwinian theories. I argue that, the opponents of directed mutation deployed an old map in their attempt to protect the authority of Darwinism. By the time of the directed mutation debate that old map had developed significant persistence and had been adopted culturally and intellectually in a number of tacit and explicit ways. This section traces the genesis and perpetuation of that old map as a way of illuminating the activity of, and motivation for, this aspect of the debate.

The genesis of the old map of Darwinism is traced here as a function of the accumulation and exercise of cultural authority by the agents of the modern synthesis in the period 1930-1960. In this period various authors became vocal advocates of Darwinism, and began a process that can be best described as boundary work for monopolization. Their expressed goal was to assert Darwinism as the primary evolutionary theory. Through a phase of recruitment, and under the auspices of various committees and societies, this group constructed the authority of Darwinism and eventually translated that authority into a persistent identity of Darwinism in the form of an old map. This section describes the rise of Darwinism to a position of persistent cultural authority.

To illustrate the genesis of the old map, I trace the boundary work for Darwinian monopoly from its beginnings in four canonical texts from the theories architects (3.1.1), through a phase of cohesion and consolidation as the Committee on Common Problems (CCP) (3.1.2), through expansion in the creation of the Society for the Study of Evolution (SSE) (3.1.2) and finally to the phase of deployment, during the Darwin centennial (3.1.3).

### 3.1.1: Seeking the origins of Synthetic Theory

The version of Darwinism recorded on the old map emerged from the ‘modern synthesis’ and is commonly termed ‘Neo-Darwinism’. Synthetic theory combined Darwin’s theory of evolution by natural selection with Mendelian genetics. It allowed biologists to work with a common theory, overcoming serious interdisciplinary differences in their early twentieth century epistemological approaches (see below). As Ceccarelli has put it: ‘[The synthesis] did not signal the triumph of one field over the other, nor was it immediately sparked by a new theory

or discovery. Instead, the synthesis was an interdisciplinary agreement that cooperation between the paradigms was both possible and desirable'.<sup>336</sup> The synthesis was as much an organisational achievement for biology as it was an epistemological transition.

The first step towards identifying the origins of the old map is to determine the period in which the synthetic movement arose. The synthesis period in modern biology is difficult to define in terms of commencement, protraction and completion. Historian Peter Bowler has analysed this period in detail.<sup>337</sup> In Bowler's view, the synthetic movement arose in the early 1900s at the end of the period that he refers to as the 'eclipse of Darwinism'.<sup>338</sup> Bowler describes how, in that period, biology had become fragmented. The rediscovery of Mendelian genetics had driven apart experimental and field biologists. In genetics laboratories Darwinism had been largely abandoned since the theory appeared to have little relevance at a molecular level; mutation was all-important and adaptation and selection provided no illumination of genetic principles. Meanwhile, naturalists and field biologists had similarly abandoned Darwinism on different grounds. They retained the Darwinian focus on geographic factors as agents of evolution, but when considering adaptation they favoured more Lamarckian mechanisms. Palaeontologists considered evolution to have occurred in a linear fashion, driven by either some Lamarckian mechanism or orthogenesis.<sup>339</sup> This divide in biology was compounded by each group's perception that their methods were superior to the others.<sup>340</sup>

During this fragmentation, Darwinism was almost abandoned. The theory only increased in value in the 1920s when population biology began to resolve the divide between geneticists and naturalists.<sup>341</sup> With that new alliance between sub-disciplines, the merit of natural selection as a force for evolution became apparent. Bowler suggests this alliance might represent the commencement of the synthetic period. He states:

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<sup>336</sup> Ceccarelli, L. (2001) *Shaping science with rhetoric: the cases of Dobzhansky, Schrodinger and Wilson*. University of Chicago Press, Chicago.

<sup>337</sup> Bowler, P. (1989) *Evolution: the history of an idea*. Revised edition. University of California Press, Berkeley, California.

<sup>338</sup> A period at the turn of the twentieth century when Darwinian theory had declined in popularity and ceased to be used by the majority of scientists.

<sup>339</sup> Bowler, P. (1989)

<sup>340</sup> Ceccarelli, L. (2001)

<sup>341</sup> Ceccarelli, L. (2001)

‘Population genetics was important not so much because it supplied totally new concepts but because it destroyed the legacy of anti-Darwinian feeling and focussed attention on new research opportunities.’<sup>342</sup>

The rise of population genetics provided a more pro-Darwinian context, in which the kind of work that would lead to synthesis became possible.

Other authors have offered alternative explanations for the rise of the synthetic movement. For example, philosopher Lindley Darden has argued that the synthesis arose because the development of knowledge in each of the dissociated disciplines had advanced to the point where the material for synthesis had become available. Only at that time when each discipline was so developed could each offer the elements required for effective synthesis.<sup>343</sup> Garland Allen argues that a conceptual transformation precipitated the synthetic period. He suggests that geneticists had to become less mechanistic and atomistic and that with this achieved they were able to perceive the holistic reality of evolution that involved populations and organisms in environments. That enabled the geneticists to perceive the merit of field naturalism and so move towards synthesis.<sup>344</sup> Conversley, historian Betty Smocovitis has argued that it was more the field naturalists who underwent conceptual change to permit the rise of the synthesis. She suggests that for synthesis to become possible the biological sciences had to move towards mechanized concepts and language, and attempt to produce theories more like those in the harder sciences. To achieve that transition the naturalists had to embrace the value of experiment and proximate causal explanation.<sup>345</sup> Finally, historian Joseph Cain has argued that the co-operation that underlay the synthesis was promoted by sociopsychological motives. He argues that field workers were losing status and funding in relation to the experimental workers and that they sought synthesis in an attempt to retain authority and ensure their inclusion in the evolutionary studies

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<sup>342</sup> Bowler, P. (1989) p.308

<sup>343</sup> Darden, L. (1986) Relations among fields in the evolutionary synthesis. In: W. Bechtel (Ed.) *Integrating scientific disciplines*. Martinus Nijhoff Publishers, Dordrecht.

<sup>344</sup> Allen, G. (1975) *Life science in the twentieth century*. John Wiley & Sons, New York.

<sup>345</sup> Smocovitis, V. (1992) Unifying biology: the evolutionary synthesis and evolutionary biology. *Journal of the History of Biology*, 25: 1-65.

community.<sup>346</sup>

There is general agreement that the beginnings of the synthetic movement can be traced to the 1920s, with the necessary precursor of population genetics emerging in that decade. However, that period does not correspond exactly with the beginning of the boundary work for monopolization that I discuss here. The 1920s provided the more pro-Darwinian context, but the boundary work that shaped the synthesis requires a more specific point of origin. To trace the origins of the old map the first acts of boundary work must be identified.

Russian Biologist Theodosius Dobzhansky's *Genetics and the Origin of Species*<sup>347</sup> appears to many authors as the text that framed and defined the modern synthetic approach.<sup>348,349</sup> I argue that in this text we see the first acts of boundary work for Darwinian monopolization. Leah Ceccarelli describes this book as:

‘...an evolutionary treatise that did more to influence interdisciplinary agreement than any other; it got the word out about developments in theory and data in the different disciplines, built a new conceptual understanding in the minds of its readers, and convinced people from different social groups that it was in their best interest to draw disciplines together.’<sup>350</sup>

And Dobzhansky biographer Mark Adams goes as far as to state:

‘Theodosius Dobzhansky...was one of the most important biologists of the twentieth century. Consider his achievements. The central architect of the modern evolutionary synthesis...he integrated diverse biological specialities in his remarkably influential classic, *Genetics and the Origin of Species* (1937) - a book that reoriented the thinking of many biologists and whose subsequent editions constituted the evolving *locus classicus* of the new view.’<sup>351</sup>

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<sup>346</sup> [See Cain, J. (1993) Common problems and cooperative solutions: organizational activity in evolutionary studies, 1936-1947. *Isis*, 84: 1-25.]

<sup>347</sup> Dobzhansky, T. (1937) *Genetics and the origin of species*. Columbia University Press, New York.

<sup>348</sup> Bowler, P. (1992) *The environmental sciences*. Harper Collins, London.

<sup>349</sup> Leah Ceccarelli has studied the impact of *Genetics and the origin of species*, and described the status awarded to the text. See Ceccarelli, L. (2001)

<sup>350</sup> Ceccarelli, L. (2001) p.24

<sup>351</sup> Adams, M. (1994) Introduction: Theodosius Dobzhansky in Russia and America. In: M. Adams

Several factors make Dobzhansky a prime candidate for architect of the modern synthesis, and founder of the old map identity of Darwinism. In the context of the fragmentation of biology Dobzhansky possessed a virtually unique breadth of learning across this divide. His training and experience as both an experimental biologist and naturalist allowed him to perceive the potential compatibility of the approaches and to describe, in his 1937 publication, what he perceived as a unified approach to biology. He had trained in the tradition of natural history in Russia, and throughout his career he conducted field research. Yet, he also learned genetic techniques in Russia and pursued laboratory based genetics research after he moved to the United States in the late 1920s.<sup>352</sup> The iconic status attached to his 1937 publication largely stems from appreciation of the unique skills that enabled Dobzhansky to transcend the dichotomy that had divided researchers.

Dobzhansky's success in 1937 was as a popularizer of Darwinian theory. He did not develop new theories in his text, but rather used his skills to enable a new presentation of existing theory. He translated the complex mathematical models of the population geneticists into a form that biologists from other disciplines could understand.<sup>353</sup> Historian Bentley Glass says of Dobzhansky's presentation that:

'...for the first time, the profound significance of the work done in population genetics in Russia and Germany was combined with an exposition of the new Neo-Darwinism stemming from R. A. Fisher, Sewall Wright, and J. B. S. Haldane...'.<sup>354</sup>

Dobzhansky himself recognised this as his key contribution, saying that synthesis was 'in the air' and that his role was to 'popularize' existing theories that were stifled by 'abstruse' and 'esoteric' mathematics.<sup>355</sup> Dobzhansky was familiar with the innovative techniques developed by Russian geneticists during the 1920s, and thought these presented an opportunity for progress in evolutionary research. When

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(Ed.) *The evolution of Theodosius Dobzhansky: essays on his life and thought in Russia and America*. Princeton University Press, New Jersey. p.3

<sup>352</sup> Ceccarelli, L. (2001)

<sup>353</sup> Ceccarelli, L. (2001)

<sup>354</sup> Glass, B. (1980) *The roving naturalist: travel letters of Theodosius Dobzhansky*. American Philosophical Society, Philadelphia. Introduction.

<sup>355</sup> Ceccarelli, L. (2001)



he moved to T. H. Morgan's 'fly room' at Columbia University in 1927 he imported these techniques along with his geneticist/naturalist balance.<sup>356</sup> After 1927 Dobzhansky's publications are dominated by reports of specific chromosomal observations in *Drosophila*. His 1937 *Genetics and the Origin of Species*<sup>357</sup> in many respects stands out from his other published material in the period, being singular in its scope and attention to the broad theories of genetics and evolution.

Dobzhansky's personal background also promoted him as a key agent in the contest between Darwinians and Lamarckians. His status as a Russian émigré indicates a possible cultural bias underlying the triumphalist narrative of Darwinism that he promoted. By 1937, when Dobzhansky's *Genetics and the Origin of Species* was published, Russian science had been debilitated by the profound impact of Lysenkoism on scientific theory and practice. Following the advent of Stalinism in the 1930s, Dobzhansky was vilified in Russia.<sup>358</sup> He was considered a traitor, and as Lysenko rose to power Dobzhansky became a focus for press attention where he was represented as "a 'fly-lover and man-hater,' a tool of machinating capitalists and American imperialism."<sup>359</sup> The disastrous national science policies promoted by Lysenko were largely bred out of adherence to Lamarckian principles. Therefore, Dobzhansky had a personal motivation for disavowing Lamarckism. To Dobzhansky the abandonment of Lamarckism as a tool for applied evolutionary work appeared as a practical necessity for the avoidance of a crisis in science; Dobzhansky had a cautionary tale to offer.

While Dobzhansky's text might represent an iconic origin of the modern synthesis in terms of its content, its favourable reception relied upon the concurrent development of a receptive audience primed for the rhetoric of disciplinary unification. Joe Cain has highlighted the widespread calls for unification across Britain and America, and described the period 1936-1947 as ripe for co-operative activities in biology.<sup>360</sup> The combination of Dobzhansky's approach and the primed audience meant that this text 'quickly and profoundly influenced evolutionary studies in America', making him one of the 'central and most influential participants

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<sup>356</sup> Adams, M. (1994)

<sup>357</sup> Dobzhansky, T. (1937)

<sup>358</sup> Adams, M. (1994)

<sup>359</sup> Adams, M. (1994) p.5

<sup>360</sup> Cain, J. (1993)

in this period of evolutionary studies'. The several editions of the text were received very well, and 'their widespread use as textbooks exemplified this broad influence'.<sup>361</sup>

Therefore, on several levels, Dobzhansky's 1937 text provides a logical place to seek the first acts of boundary work and the origins of the old map. I argue that Dobzhansky's boundary work strategy relied upon two devices: first, the promotion of Darwinism as a single theory that might unite the biological sciences, second, the concurrent rejection of Lamarckism as a viable alternative. In *Genetics and the Origin of Species* we see this strategy outlined and put into action.

In the following sections I describe four foundational texts of the modern synthesis, beginning with Dobzhansky's, to illustrate the boundary work project of the synthesis architects.<sup>362</sup> It should be noted that the search for boundary work in these canonical texts does not assume that these authors were from the very start acting specifically as a corporation for the advancement of Darwinism. In this first phase of synthesis these iconic texts were produced perhaps more in service to individual interests, than to a self-conscious programme for Darwinian monopoly. There was a general consensus within biology that the disparate areas of field and laboratory science should be united.<sup>363</sup> Dobzhansky offered Darwinism as a principle for unification. The three subsequent texts that I describe (Huxley 1942, Mayr 1942, and Simpson 1944) follow Dobzhansky's lead, but with important motivations beyond that of Darwinian expansion. In particular, Mayr was keen for his specialist area of taxonomy or systematics to be included in the synthesis, while Simpson was keen that paleontologists not be left out. Dobzhansky offered a suggested style for achieving synthesis, and Huxley, Mayr and Simpson followed at least partly pragmatically. Initially, as the authors pushed for authority, their primary concern was perhaps just to be attached to it.

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<sup>361</sup> Cain, J. (1993) p.9

<sup>362</sup> The analysis I offer here of these canonical texts is not intended to recommend this 'boundary work for synthesis and expansion' reading as the only one possible. I follow Cain's caution that these texts should be considered to have polyvalent readings and associated meanings. [Cain, J. (2003) A matter of perspective: multiple readings of George Gaylord Simpson's 'tempo and mode in evolution'. *Archives of Natural History*, 30(1): 28-39] In this project, my search for boundary work is a methodological device for recovering the emergence of trends in evolutionary biology that I wish to trace to the late twentieth century.

<sup>363</sup> Cain, J. (1993)

The first acts of boundary work for monopolization: Theodosius Dobzhansky and *Genetics and the Origin of Species* (1937)

Dobzhansky's 1937 text introduces the new framing of Darwinism and Lamarckism that I argue became central to the boundary work of the synthesis architects; his narrative promoted Darwinism, while neglecting and tacitly defaming Lamarckism. Dobzhansky presents Darwinism as a tool for forging a union between geneticists and naturalists, offering to mend the problematic divide in the life sciences. For Dobzhansky, the problem was that naturalists largely adhered to Lamarckism and geneticists largely overlooked the importance of Darwinian theory. Dobzhansky offered to mend this rift by indicating to the latter group the value of Darwinism to their work, while presenting the Lamarckism of the former group as an unfortunate outcome of the disciplinary divide.

The main body of the work is a restatement of Darwinian theory in light of Dobzhansky's special skills bridging this divide. Mayr has observed that: 'In a way, the synthesis was nothing but confirmation of Darwin's original theory...'<sup>364</sup> In this text we see Dobzhansky reiterating Darwinian theory in relation to developments in twentieth century science. Ceccarelli has said that Dobzhansky's challenge was to 'lay out the theory that supported the possibility of collaboration', and 'inspire each side to believe that cooperative action was in its own best interest'.<sup>365</sup> He achieved this aspect of his project through simplifying complex mathematics, providing examples of field work proofs for mathematical theories, introducing map metaphors to bolster population genetics predictions, surveying new data and collating research findings, directly addressing the reservations naturalists' had concerning the validity of laboratory study and addressing geneticists concerns regarding the use of field work.<sup>366</sup> He did all this whilst also assuring 'each side that he supported their core beliefs'.<sup>367</sup>

Meanwhile, Dobzhansky presents the extraction of Lamarckism from evolutionary theory as a practical necessity for the new approach:

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<sup>364</sup> Mayr, E. (1998) p.xiii

<sup>365</sup> Ceccarelli, L. (2001)

<sup>366</sup> Ceccarelli, L.. (2001)

<sup>367</sup> Ceccarelli, L. (2001) p.40-41

‘Considerations of space have forced us to refrain from a detailed discussion of some of the objections that have been advanced against the genetic treatment of evolutionary problems. Thus, Lamarckian doctrines find but a brief mention. The treatment had to be made assertive rather than polemic, dogmatic rather than apologetic.’<sup>368</sup>

The removal of Lamarckism as an opposition to Darwinism serves a dual purpose. Firstly, this approach has the immediate effect of tacitly asserting the authority of Darwinism, suggesting that there is no viable alternative to the theory and so implying that inevitability recommends its uptake. For Dobzhansky that inevitability would be expressed ‘assertively’ and ‘dogmatically’. Secondly, the extraction of Lamarckism as an antithesis to Darwinian work has the long-term effect of underpinning the emerging triumphalist narrative of Darwinian history; the authority of Darwinian theory is reinforced by the sense that it has triumphed over Lamarckism. The apparent necessity of by-passing Lamarckism, which Dobzhansky describes as a consideration of ‘space’, actually functions as a rhetorical device underpinning his singular attention to Darwinian theory.

By constructing biologists’ pluralistic approaches to evolution as a side-effect of disciplinary divide, Dobzhansky eliminates the need to refute Lamarckism. In his framing, Darwinism is an all-purpose tool that requires no bolstering from alternative theories. Thus, Lamarckism doesn’t need to be refuted, but rather abandoned for pragmatic purposes. Dobzhansky offers a ‘get out’ to Lamarckian supporters, allowing them to recognise their use of Lamarckism as an unavoidable outcome of a wider divide in biology, rather than a defining feature of their work.

This formative text recommends a boundary work strategy for the expansion of the authority of biology, evolutionary theory and Darwinism. Dobzhansky recommended first the broad application of Darwinism, and second, the omission of alternative theories (especially Lamarckism). The result is an early version of the triumphalist representation of Darwinism – it appears as an unchallenged theory of evolution. In this text Lamarckism is ignored, only later does the stigmatization and

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<sup>368</sup> Dobzhansky, T. (1937) Preface

active defamation of the theory begin (see section 3.2). At this stage, Dobzhansky's principal goal was to increase the authority of biology by mending internal rifts (with Darwinism as a tool for that project). Only later, once the rifts are mended, does the drive to assert Darwinian monopoly become a motivation in itself.<sup>369</sup>

To appreciate the extent to which Dobzhansky's approach was deployed in the contest for authority, it is essential to appreciate the influence his text had upon other agents of the synthesis, and the biological community in general. Several authors have addressed the texts' influence. Biologist William Provine says that it was the most influential evolutionary text of the twentieth century, and that:

'...it was required reading for all evolutionists (I have yet to find an evolutionist trained in the United States between 1937 and 1960 who did not read the book)...'<sup>370</sup>

Ernst Mayr, fellow synthesis architect and historian of evolutionary theory, has asserted that:

'There is complete agreement among the participants of the evolutionary synthesis as well as among historians that it was one particular publication that heralded the beginning of the synthesis, and in fact was more responsible for it than any other, Dobzhansky's *Genetics and the origin of species*.'<sup>371</sup>

Biologist Steven Jay Gould has gone as far as to say that:

'...his book had been the direct instigator of all volumes that followed.'<sup>372</sup>

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<sup>369</sup> Mayr has suggested that there was significant consensus between biologists by the time the 1947 Princeton conference concluded the work of the Committee on Common Problems of Genetics, Paleontology and Systematics. [See Mayr, E. (1980a)]

<sup>370</sup> Provine, W. (1981) Origins of the genetics of natural populations series. In: R. Lewontin *et al* (Eds.) *Dobzhansky's genetics of natural populations I-XLIII*. Columbia University Press, New York. p.74

<sup>371</sup> Mayr, E. (1982) *The growth of biological thought: diversity, evolution and inheritance*. Harvard University Press. p.569

<sup>372</sup> Gould, S. J. (1982) Introduction. In: T. Dobzhansky, *Genetics and the origin of species*. Columbia University Press, New York. p.xxvii

### Julian Huxley and *Evolution: The Modern Synthesis* (1942)

A second book frequently cited as a foundational text of the modern synthesis is Julian Huxley's *Evolution: The Modern Synthesis* (1942).<sup>373</sup> Like Dobzhansky, Huxley had some unique qualities as a biologist in this period. His academic interests were eclectic. He had a keen interest in behaviour, as well as embryology and systematics. He had trained as a zoologist, with an interest in ornithology. He was a firm believer in the value of education, following the humanist tradition throughout his life, and producing popular works from early in his career.<sup>374</sup> Zoologists Paul Harvey has stated that Huxley's legacy is 'more one of inspiration than of scientific achievement.'<sup>375</sup> His interest in popularisation made him a valuable recruit for the synthesis movement, which would require popularisation in order to assure its status within cultural cartography.<sup>376</sup>

Huxley bore the legacy of his grandfather, Thomas Henry Huxley. T. H. Huxley had been Charles Darwin's greatest defender in the period immediately subsequent to the publication of *The Origin Of Species*.<sup>377</sup> The dichotomy of naturalists and geneticists, that had inspired so much Lamarckian support in field naturalists, mirrored a divide in Julian Huxley's own career between the legacy of his Darwinian advocate grandfather and the abandonment of Darwinism manifest in some of his own research interests (i.e. field ornithology).

Huxley coined the term 'modern synthesis' in this text, providing definition for the movement through the process of naming. Huxley introduced the book as part of a new concerted reform in evolutionary science, stating that:

'The time is ripe for an advance in our understanding of evolution. Genetics, developmental physiology, ecology, systematics, palaeontology, cytology,

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<sup>373</sup> Huxley, J. (1942) *Evolution: the modern synthesis*. George Allen & Unwin Ltd, London.

<sup>374</sup> Huxley, J. (1926) *Essays in popular science*. Chatto & Windus, London; Wells, H., Huxley, J. & Wells, G. (1930) *The science of life*. Waverley Book Company, London.

<sup>375</sup> Harvey, P. (1990) Review: The legacy of Julian Huxley – 'evolutionary studies' edited by M. Keynes and G. Ainsworth. *New Scientist*, 1715.

<sup>376</sup> Claims for cultural authority have to be articulated to those in adjacent territories before boundary delineations can be finalized; making the ability to communicate the authority claims to a wider audience a valuable skill.

<sup>377</sup> Darwin, C. (1859)

mathematical analysis, have all provided new facts and tools for research: the need to-day is for a concerted attack and synthesis.'<sup>378</sup>

Like Dobzhansky, Huxley placed emphasis on the need to unite the disparate fields in biology; acting as a British counterpart to Dobzhansky in his role as ambassador of unification.<sup>379</sup> Cain has suggested that Huxley's key contribution to the synthesis project was as a publicist of the coherent and communal approach to biology.<sup>380</sup> His boundary work is apparent in rhetoric such as 'advance' and 'attack'. Like Dobzhansky, Huxley perceived the route to reunification to be via the broad application of Darwinism. He says that:

'Biology at the present time is embarking upon a phase of synthesis after a period in which new disciplines were taken up in turn and worked out in comparative isolation. Nowhere is this movement towards unification more likely to be valuable than in this many-sided topic of evolution; and already we are seeing the first-fruits in the re-animation of Darwinism.'<sup>381</sup>

Huxley follows Dobzhansky's approach to the divide in biology; he does not reprimand non-Darwinian groups. Again, adherence to alternative theories is framed as an unavoidable effect of disciplinary division. Again, the solution is apparently simple; if biology is united then the value of Darwinism is obvious. Only division has made alternatives appear valuable. Unlike Dobzhansky, Huxley does not justify the removal of Lamarckism from his summary of evolutionary theory; he just leaves it out. His text makes only a handful of references to Lamarckism. To Huxley, the

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<sup>378</sup> Huxley, J. (1942) p.8

<sup>379</sup> Huxley had been involved alongside Dobzhansky in an effort to cohere the disparate specialists in biology through the formation of the Society for the Study of Speciation in 1939. This was intended as an informal body for the exchange of ideas and information. [Cain, J. 1993] It was short lived, despite efforts by Mayr to intervene and galvanise the project. The society has been examined by historian Joe Cain [Cain, J. (2000) Towards a 'greater degree of intergration': the Society for the Study of Speciation 1939-1941. *The British Journal for the History of Science*, 33: 85-108] Although, as Cain acknowledges, it was too short lived to 'merit a claim for major impact within the community' (Cain, J. 2000 p.85), I would still argue that it demonstrates early boundary work by the key architects of the synthesis. They later succeeded in forming a more successful society to consolidate their efforts in the form of the Committee on Common Problems in Genetics and Paleontology (see below).

<sup>380</sup> Cain, J. (1993)

<sup>381</sup> Huxley, J. (1942) p.13

removal of Lamarckism is not a consideration of 'space' as Dobzhansky proposed in 1937, nor is it a part of an emergent agenda that has to be clarified (as is the inference of Dobzhansky's justification of omission). In Huxley's treatment, the tacit omission of Lamarckian theory reflects a tenet of the new movement he has named. This shift towards the obfuscation of Lamarckism as an opposition to Darwinism would later contribute to the historical appearance of Darwinian triumph over Lamarckism. This approach implies that no further debate of the conflict between Lamarckism and Darwinism is required. Following Dobzhansky's recommendations regarding 'space', Huxley simply says: 'Nor need I go in detail through the wearisome discussion of the various scientific 'proofs' of Lamarckian inheritance that have been advanced.'<sup>382</sup> In terms of his boundary work strategy, Dobzhansky had found a recruit in Huxley.

#### Ernst Mayr and *Systematics and the Origin of Species* (1942)

During the 1940s much of the increase in the disciplinary authority of Darwinism resulted from the success of the early authors, such as Dobzhansky, in the recruitment of vocal supporters. The publication of Ernst Mayr's *Systematics and the Origin of Species* (1942)<sup>383</sup> provides an example of this vital early recruitment. Mayr's career began in his native Germany, with his first publication appearing in 1923. His early papers appeared in German and were exclusively focussed on ornithology. Mayr was primarily a field investigator and the majority of his pre-1930s papers report fieldwork expeditions. Mayr's classifications were grounded in taxonomy. In the context of his practical work in field biology 'species' for Mayr were a unit manifest in taxonomic difference rather than a theoretically problematic grouping *per se*. Mayr carried out his research without recourse to a theory of genetics, and as such represented the genetics versus naturalist divide. In 1936, as Dobzhansky prepared *Genetics and the Origin of Species* for publication, Mayr attended Dobzhansky's Columbia lectures and was impressed by the notion of

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<sup>382</sup> Huxley, J. (1942) p.458

<sup>383</sup> Mayr, E. (1942) *Systematics and the origin of species*. Columbia University Press, New York.



uniting geneticists and naturalists.<sup>384</sup> Mayr began to collect reprints of basic genetics publications and sought to teach himself this aspect of biological science. During this phase of intellectual development Mayr was encouraged and guided by frequent correspondence with Dobzhansky, with whom he shared a growing association.<sup>385</sup> In the late 1930s Mayr adopted Dobzhansky as his mentor, and after reading *Genetics and the Origin of Species* he became a disciple of the new programme for united biology. He reported that Dobzhansky's 1937 text 'delighted' him.<sup>386</sup>

In 1939, Mayr published a paper on sex ratios in birds (Mayr, 1939), putting into practice for the first time the genetics skills he had accumulated. Meanwhile, Dobzhansky moved to the Columbia zoology department, putting Mayr and himself together in New York.<sup>387</sup> Mayr also began to attend the Cold Spring Harbour Symposia, which at the time represented the United States' premier genetics meetings.<sup>388</sup> By 1943 Mayr had begun his own genetics research using *Drosophila*, with plans to extend his studies to pigeon stocks, as a way of bringing his new skills back to bear on his passion of ornithology. As he made this transition towards genetics, Mayr was also being recruited to Dobzhansky's synthesis programme. He was becoming a Darwinian, and following the programme Dobzhansky had laid out in *Genetics and the Origin of Species*. Mayr's intellectual transition was exemplified in his 1942 publication, *Systematics and the Origin of Species*. The introduction to that text was provided by Dobzhansky, in which he states:

'During the last decade the conclusions reached by many of the specialists have begun to converge towards a set of general principles applicable to the entire realm of living matter. One can only hope that this will occur in increasing measure in the future. Biology, it seems, is no longer in its childhood, as a science, it is approaching its maturity.'<sup>389</sup>

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<sup>384</sup> Cain, J. (2002) Epistemic and community transition in American evolutionary studies: the 'Committee on Common Problems in Genetics, Paleontology and Systematics' (1942-1949). *Studies in History and Philosophy of Biological and Biomedical Sciences*, 33: 283-313.

<sup>385</sup> Cain, J. (2002)

<sup>386</sup> Mayr, E. (1980a) How I became a Darwinian. In: E. Mayr & W. Provine (Eds) (1980)

<sup>387</sup> Cain, J. (1993)

<sup>388</sup> Cain, J. (2002) Also between 1939 –1941 Mayr became involved alongside Dobzhansky and Huxley in an attempt to form the Society for the Study of Speciation. See Cain, J. (2000) and footnote 46 of this text.

<sup>389</sup> Dobzhansky, T. (1942) Introduction. In: Mayr, E. (1942) *Systematics and the origin of species*.

For Dobzhansky, this maturity was an outcome of the unification that he had advocated. In writing this introduction Dobzhansky identified Mayr as one of the group of specialists who have begun to forge unification; the unifying principle being the broad application of Darwinian theory to biological research. Dobzhansky effectively acknowledged Mayr's recruitment in that introduction, identifying him as an ally in the programme for authority extension. As Cain has put it: 'In Mayr, Dobzhansky knew he had a bulldog'.<sup>390</sup>

On Mayr's part, the title of his book acts as a signal of his joining the movement; Dobzhansky wrote *Genetics and the Origin of Species* (1937) and five years later his disciple Mayr added *Systematics and the Origin of the Species* (1942). In Mayr's preface he asserts the link more vocally, stating:

'I am indeed indebted to Th. Dobzhansky [...], who aided in countless ways in the preparation of the manuscript and who encouraged and inspired me throughout.'<sup>391</sup>

By 1942 Mayr had become one of the dedicated synthesis architects that were cohered at the Columbia zoology department. That group included Dobzhansky and Simpson.<sup>392</sup> Since 1940, Mayr had 'worked aggressively' alongside them to construct 'a common-problems research community in evolutionary studies and to establish themselves at that community's centre'.<sup>393</sup>

Despite Mayr's location within the synthesis cohort at Columbia, and the appearance in his introduction that his recruitment is complete, the main text of *Systematics and the Origin of the Species* tells a slightly different story. Like Dobzhansky, Mayr refrains from discussion of Lamarckian alternatives to Darwinian evolutionary theory. In that regard he appears to have assumed the device that Dobzhansky recommended in 1937. However, in places, his earlier career as a naturalist marks his treatment. For example, Mayr states that:

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Columbia University Press, New York. p.vii

<sup>390</sup> Cain, J. (2002) p.295

<sup>391</sup> Mayr, E. (1942) p.x

<sup>392</sup> Cain (1993) has called this group the 'New York Circle'.

<sup>393</sup> Cain, J. (1993) p.10

‘The opinion was formerly widespread among taxonomists (including even Darwin) that the germ plasm can react to the needs of the body.’<sup>394</sup>

And adds to this:

‘...it seems premature to assert with too much positiveness that all gene mutation is strictly random.’<sup>395</sup>

Irrespective of these occasional allusions to Lamarckian views, Mayr’s text overall represented a further forceful publication in favour of the synthesis programme. Despite his allusion to non-random mutation, Mayr does not go as far as to mention Lamarckism. As Burkhardt puts it: ‘...Mayr did not even bother to reject Lamarckism...he simply did not refer to it.’<sup>396</sup> In that sense, Mayr’s approach follows Dobzhansky’s strategy.

In subsequent years Mayr gained further influence as a member of the synthesis movement, especially through his role in the Committee on Common Problems in Genetics, Palaeontology and Systematics (discussed below). His dedication to Darwinism became more refined and despite his early concerns (reflected to a degree in his 1942 publication) he became one of the most enduring and ardent advocates of Darwinism in the twentieth century. Certainly, Mayr was a valuable early recruit to the movement for synthesis and to the authority struggle that had begun.

#### George Gaylord Simpson and *Tempo and Mode in Evolution* (1944)

In 1944 Simpson published *Tempo and Mode in Evolution*.<sup>397</sup> In that text, he allied himself with the synthesis movement and attempted to contribute hard theoretical material to the programme that Dobzhansky and Huxley had begun. Walter Fitch and Francisco Ayala have examined Simpson’s motivation, and state

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<sup>394</sup> Mayr, E. (1942) p. 67

<sup>395</sup> Mayr, E. (1942) p. 68

<sup>396</sup> Burkhardt, R. (1980) Lamarckism in Britain and the United States. In: E. Mayr & W. Provine (Eds.) *The evolutionary synthesis*. Harvard University Press, Massachusetts. p.349

<sup>397</sup> Simpson, G. (1944) *Tempo and mode in evolution*. Columbia University Press, New York.

that he authored his 1944 work self-consciously in the wake of the Dobzhansky's 1937 text, acknowledging its influence on his own work, saying that: it 'profoundly changed my whole outlook'.<sup>398,399</sup> He not only shared Dobzhansky's aim of uniting genetics with natural selection, but also subscribed to the principle of Darwinian unification that Dobzhansky had advocated. Simpson had been a Darwinian enthusiast since his college days, and had 'found in Darwin a point of view, a philosophical stance, that resonated with his own.'<sup>400</sup>

Fitch and Ayala are keen to indicate that, much of the success of Dobzhansky's *Genetics and the Origin of Species* had been his success at writing '...in prose that biologists could understand.'<sup>401</sup> Simpson's *Tempo and Mode in Evolution* added rigour to this with its technical account of evolutionary dynamics. In the introduction Simpson emphasised the merit of a theoretical and technical approach to synthesis, stating that:

'Facts are useless to science unless they are understood. They are to be understood only by theoretical interpretation...The one merit that is claimed for this study is that it suggests new ways of looking at facts and new sorts of facts to look for.'<sup>402</sup>

In this statement Simpson reasserts the value of using a theoretical programme (Darwinism in this case), both for better understanding existing work, and for achieving learning in the future.

Simpson's approach to Lamarckism in this text is very much in line with Dobzhansky's recommendation. Firstly, Simpson barely mentions Lamarckian theory at all. When he does address the issue he states briefly that:

'Experiments in the present century...not only have failed to corroborate that there is

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<sup>398</sup> Fitch, W. & Ayala, F. (Eds.) (1995) *Tempo and mode in evolution: genetics and paleontology 50 years after Simpson*. National Academies Press. p.iii

<sup>399</sup> Cain, J (1993) has attributed Simpson, and Mayr's contributions to synthesis in part to their professional interests. In that reading, Simpson 'self-consciously authored in the wake of Dobzhansky' (as did Mayr) to ensure that their specialisms be included in the powerful new synthesis. They wanted to ensure the future of Paleontology and Systematics respectively. Therefore, part of their boundary work was in service to disciplinary interests.

<sup>400</sup> Laporte, L. (1990) The world into which Darwin led Simpson. *Journal of the History of Biology*, 23(3): 499-516.

<sup>401</sup> Fitch, W. & Ayala, F. (Eds.) (1995) p.iv

<sup>402</sup> Simpson, G. (1944) p.xviii

such a process [as Lamarckian evolution] but also have shown that it is highly improbable if not impossible.<sup>403</sup>

As historian Richard Burkhardt observes: in Simpson's analysis 'Lamarckism was not so much disproved as discarded'.<sup>404</sup> True to Dobzhansky's boundary work strategy, Simpson pursues synthesis, advocating Darwinism, while omitting Lamarckism.

These contributions from the primary architects of the synthetic programme highlight key points of interest in this early period. Firstly, we see that a vital initial step towards synthetic theory was the establishment of an agenda and associated programme for authorship. Dobzhansky (1937), and subsequently Huxley (1942), achieved this through the publication of simple, yet forceful and influential, texts. Secondly, we see how this programme began to be translated into a broader project, as first a new and vocal author is recruited (Mayr) and second, complexity is added back by Simpson (1944). Historically, these examples are not isolated contributions from these authors. Each of these authors made other contributions in the period, both to the synthetic approach and also in their individual fields. Also, of course, many other authors contributed to the early synthesis period.

The selection of texts described above serves to expose tenets of the emerging modern synthetic programme, illustrating the boundary work activities that had begun to drive towards Darwinian monopoly. As a pragmatic device of the unificationists' a certain approach to the presentation of Darwinism and Lamarckism emerged. Dobzhansky made that approach explicit in his 1937 text, and Huxley, Mayr and Simpson followed his recommendations – to broadly apply Darwinism 'assertively' and 'dogmatically' whilst excluding alternative theories, namely Lamarckism. An identity of Darwinism and Lamarckism began to emerge that would later inform the old map of Darwinian authority.

The next phase of Darwinian expansion involved the spread of the synthesis programme and its attached authorial approach through further recruitment and publicity. In this phase of scale change we see the importance of the move away

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<sup>403</sup> Simpson, G. (1944)

<sup>404</sup> Burkhardt, R. (1998) p.349

from individual authorship, and the trend towards a growth in authority under the auspices of larger associations and communities.

### 3.1.2: The growth of the synthetic movement

As Huxley's *Evolution: The Modern Synthesis* was being printed (1942) and reprinted (1943) a further consolidation of the new movement was occurring. On the 6<sup>th</sup> of February 1943 the US National Academy of Science 'Committee on Common Problems of Genetics and Palaeontology' (CCP) was established.<sup>405,406</sup> This committee was a joint body set up by the Division of Geology and Geography and the Division of Biology and Agriculture of the National Research Council (NRC).<sup>407</sup> The CCP's core membership was largely comprised of architects of the modern synthesis.<sup>408</sup> Simpson and Dobzhansky had worked together since 1941 to press the NRC to set up the committee.<sup>409</sup> The formation of this committee was an important act of boundary work; it consolidated the synthesis programme, and was a powerful tool for the recruitment and scale change that monopoly required.

The stated aim of the CCP was to forge links between sub-disciplines, eliminating the problematic divisions that Dobzhansky, Huxley and Mayr had highlighted. In particular, the committee was purposed to bring paleontologists, who had feared being left behind as the wave of experimentalism swept away the less desirable field and museum sciences, in line with the synthesis programme. Simpson, along with fellow paleontologists Horace Wood and Glenn Jepsen had acted to ensure their specialism's inclusion in the CCP project.<sup>410</sup>

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<sup>405</sup> Although the CCP was not the only committee established in the late 1930s and early 1940s as part of the synthesis project, its endurance and increasing support make it the most relevant example to this narrative of increasing intellectual authority. (see also footnote 45)

<sup>406</sup> After 1944 the committee was renamed 'The Committee on Common Problems in Genetics, Paleontology and Systematics'. Cain, J. (1993)

<sup>407</sup> Jepsen, G., Simpson, G. & Mayr, E. (Eds.) (1949) *Genetics, paleontology and evolution*. Princeton University Press, New Jersey.

<sup>408</sup> Of particular note: The committees divisional chairmen were: Theodosius Dobzhansky (section on genetics), Ernst Mayr (section on systematics), G. Ledyard Stebbins (Vice-Chairman Western Group) and George Gaylord Simpson (overall chairman) [Jepsen G., Simpson, G. & Mayr, E. (Eds.) (1949) p.xi].

<sup>409</sup> Cain, J. (1993)

<sup>410</sup> Cain, J. (2002)

The materials generated by this committee aimed to emphasise the links between sub-disciplines, and to subject the work of disparate groups to Darwinian theory;<sup>411</sup> effectively Dobzhansky's 1937 recommendations in action. This approach brought the work of ecologists, systematists, field naturalists and geneticists within the remit of the modern synthesis, and thus by definition, into the remit of Darwinism. Cain suggests that the real work of this period can be considered to have been the development of a new 'epistemic community'<sup>412</sup>; an activity that underwrites the architects' bid for increased disciplinary authority and reinforces their boundary work. In service to that new epistemic community the CCP acted as a tool for 'assisting the transition of recruits into the fold and for pressing further with the harvest of new fruits'.<sup>413</sup> Boundary work for expansion had started to take effect.

This committee was intended as a temporary alliance of interested individuals with the goal of carrying out the problematic, but apparently necessary, unification of the biological sciences. The committee remained active despite wartime conditions, and bulletins of correspondence ensured its survival.<sup>414</sup> Ernst Mayr edited these bulletins, and thus acquired a powerful forum for the deployment of his own growing Darwinian agenda. As Cain has put it: 'Mayr was an aggressive and invasive editor: prompting queries, recruiting additional materials, instigating interaction.'<sup>415</sup> The CCP galvanised and deployed the synthesis programme, and as Cain had pointed out: 'bulletin correspondence often articulated a community identity and helped define shared beliefs'.<sup>416</sup>

The architects of the synthesis continued to press for expansion. In 1944 Simpson had returned from war, wanting to make changes at the CCP and extend its influence.<sup>417</sup> Cain has noted of the architects that: '...seeking organisational expansion, they signalled a desire to raise their project to a level comparable with

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<sup>411</sup> The largest collection of these papers appears in: Jepsen, G., Simpson, G. & Mayr, E. (Eds.) (1949)

<sup>412</sup> Cain, J. (2002) Epistemic communities have been described by Knorr Cetina [Knorr Cetina, K. (1999) *Epistemic cultures: how the sciences make knowledge*. Harvard University Press, Cambridge, MA.]

<sup>413</sup> Cain, J. (2002) p.309

<sup>414</sup> Jepsen, G., Simpson, G. & Mayr, E. (Eds.) (1949)

<sup>415</sup> Cain, J. (2002) p. 304

<sup>416</sup> Cain, J. (1993) p. 12

<sup>417</sup> Cain, J. (1993)

other well-established ventures in biology'.<sup>418</sup> The architects were pressing for disciplinary authority as part of their boundary work for expansion. Their individual interests had been served through the CCP, and systematics and paleontology had a more secure place in the synthesis, as Mayr and Simpson had been keen to ensure. Through the CCP the scale of the synthesis movement had been increased, and its influence extended. Cain suggests that: 'The CCP seems ideally described as an incipient moment in the expansion of this community...a foothold for efforts towards a professional society and journal'.<sup>419</sup> The 1947 Princeton conference that would conclude the work of the CCP 'acted as a kind of debutants' ball for the epistemic community'.<sup>420</sup> That community was now ready to deploy its epistemic approach more widely. The next stage for the Darwinians was to push for more authority within science, and across the cultural cartography.

The work of the CCP was summarised in a 1947 meeting at Princeton, and the committee's recommendations and outcomes were published in 1948<sup>421</sup> and 1949<sup>422</sup>. This meeting, and the publications it generated, is often cited as the completion of the terms and content of the modern synthesis. It also marks the achievement of consensus within biology regarding the increased authority of Darwinism. In Mayr's discussion of the 1947 meeting he remarks that: '...it was almost impossible to get a controversy going, so far reaching was the basic agreement among the participants.'<sup>423</sup> He adds that: 'It was not that the synthesis was hammered out during the Princeton conference – rather, the conference constitutes the most convincing documentation that a synthesis had occurred during the previous decade.'<sup>424</sup>

The conference can also be viewed as the completion of the first phase of the boundary work that would ultimately create the old map of the authority of Darwinism. In the period 1937-1947 the foundations of Darwinian monopoly had

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<sup>418</sup> Cain, J. (1993) p. 13

<sup>419</sup> Cain, J. (2002) p.309

<sup>420</sup> Cain, J. (2002) p.309

<sup>421</sup> Jepsen, G. (1948) *Genetics, paleontology, and evolution*. Princeton University Press, Princeton, NJ.

<sup>422</sup> Jepsen, G., Simpson, G. & Mayr, E. (Eds.) (1949) *Genetics, paleontology, and evolution*. Princeton University Press, Princeton, New Jersey.

<sup>423</sup> Mayr, E. (1980a) Prologue: some thoughts on the history of the evolutionary synthesis. In: Mayr & Provine, (Eds.) (1980) p.42

<sup>424</sup> Mayr, E. (1980a) p.42



been laid; the merit of Darwinism had been asserted, Lamarckism had been extracted as an opposition, and the 'modern synthesis' had become 'evolutionary biology'. That had been achieved through a handful of canonical texts, the bulletins of the CCP, and recruitment under the auspices of the CCP. An appearance of Darwinian predominance, and a ratifying historical account of the accomplishment of that predominance, had been set in place. The message of the architects of the synthesis was established and already being disseminated in their publications; they had created what Knorr-Cetina would call an 'epistemic community'.<sup>425</sup>

The committee's collected works in *Genetics, Palaeontology and Evolution* (1949) highlight the broad utility of Darwinism across the biological sciences. They do not refer to alternative evolutionary theories in detail. Lamarckism receives barely a mention amongst the 23 papers that comprise the volume. This omission reflects Dobzhansky's recommendation; that Lamarckism should be omitted due to considerations of 'space'. Adherence to that approach was perhaps ensured by the fact that Dobzhansky was one of the chairmen of the committee.

The status of Dobzhansky and other of the synthesis architects within the CCP illustrates how, through leadership, the personal agendas of some individuals became reflected in the broader ideology of this group of theorists. We see how a few individuals, dedicated to the promotion of Darwinism, and extraction of Lamarckian opposition, were able to use their roles within the CCP for the deployment of their agenda to a wider audience. The boundary work of a few individuals was translated to become the strategy adopted across biology as various interest groups jostled for increased authority. The formation of the CCP made the increase in scale of the synthesis movement official, and provided a context for the recruitment and training of new members.

In March 1946, a new society had been formed from the committee to ensure that what begun as a temporary programme for reform would have longer-term influence in the development of evolutionary biology. The original committee had aimed to solve an apparently immediate problem for the future of evolutionary

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<sup>425</sup> See: Knorr Cetina, K. (1999) Cetina's epistemic communities share methodology, and use the same tools and language. They have common standards, and approach their work in a particular way. In the strong version, the epistemic community shapes the understandings and identities of its members. This provides an apt description of the community that emerged in the wake of the CCP.

biology (the division of disciplines), and the new society took over once the programme extended in scope from problem-solving to the management of the new evolutionary programme that arose from unification. The new society was named 'The Society for the Study of Evolution' (SSE), and a grant from the American Philosophical Society ensured its permanence. After lobbying by Huxley the journal *Evolution* was established to present the society's work, and its first edition appeared in 1947 with Mayr as editor.<sup>426</sup> What had started as a programme for unification, had grown into evolutionary biology. The modern synthesis became a corner stone of the biological sciences, and thus so too was Darwinian theory. Boundary work for the expansion of the authority attached to Darwinian theory had become boundary work for the monopolization of evolutionary biology.

The SSE had not been formed by the broad community of individuals working on evolutionary problems in the period, but rather by a discrete partisan group with a defined goal of promoting Darwinian theory and eliminating Lamarckian debate. The SSE became their permanent manifestation. With this transition, what had been the agenda of a small group of unificationists had become the agenda of the dominant movement in evolutionary biology. As Cain has noted: 'Effectively in control of the SSE and fairly clear about the sort of work that interested them, members of the New York Circle were able to define a mainstream in American evolutionary studies largely based on their own interests.'<sup>427</sup> The effect of this was to render Darwinism the primary theory of evolution, carried to dominance by the advocacy of a group that had become hugely influential and had undergone significant scale change. In addition, the neglect of Lamarckism, which had been enforced through the agenda of this group, became a *status quo* in evolutionary biology.

Thus, influence and voice of a handful of key individuals was first enhanced by their cohesion as the CCP, and later increased by their self-styling as the mainstream of evolutionary biology (as the SEE). The architects of the synthesis had won and constructed authority for their movement. The naming of the new committee as the society for the 'evolution' was a powerful rhetorical device

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<sup>426</sup> Cain, J. (1993)

<sup>427</sup> Cain, J. (1993) p. 23

implying their monopoly of the cultural authority available in evolutionary biology. Implied authority can be an important boundary work device.

Meanwhile, subsequent to the formation of the SSE some of the individual architects of the synthesis continued to press for expansion across the cultural cartography. Bowler has noted that some authors attempted to 'extend the new Darwinism into a general world view'. In particular, Simpson (1949) and Huxley (1953) offered texts that were intended to 'go beyond the technical details of the theory into a broader vision of the nature and purpose of life'.<sup>428</sup>

It is useful to consider the increase in scale and extension of authority of the synthesis agents in the terms of some established models of group activity in science. Highlighting scale changes within the synthesis movement throughout the 1930s, 1940s and 1950s reveals the extension of intellectual and cultural authority that was achieved. In particular, the increasing membership of the synthesis group indicates how a change in scale was able to confer the increase in authority that allowed the synthesis group to achieve monopoly. Also, consideration of the leadership structure within this growing group indicates how the agendas of a few individuals from the 1930s and early 1940s period were able to achieve widespread influence. Collins' 'core-set' theory is perhaps the most useful tool for exploring these dynamics.<sup>429</sup>

In the case of the early synthesis authors, the designation 'core-set' reflects the international/multi-institutional composition of the group and the discrete nature of their intellectual programme. In the late 1930s and early 1940s context of disciplinary divide these individuals undertook a cohesive, forceful assertion of the authority of Darwinism. Collins' methodology describes grouping during controversial episodes, and using it to describe the synthesis group reminds us that their early work was indeed contentious. Their strong advocacy of Darwinism emerged in a context of Darwinian eclipse; they were attempting to resurrect a theoretical approach that many researchers had abandoned. The controversial nature of this programme is even more apparent when we consider that Neo-Lamarckism

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<sup>428</sup> Bowler, P. (1983) p.325 [and see: Simpson, G. (1949) *The meaning of evolution*. Yale University Press, New Haven; Huxley, J. (1953) *Evolution in action: based on the Pattern Foundation Lectures delivered at Indiana state University in 1951*. Chatto & Windus, London.]

<sup>429</sup> See Collins, H. (1981b); Collins, H. (1985). The core-set is introduced in this project in Chapter 2.

had gained support and authority during the period of the eclipse and was in fact being used by perhaps the majority of evolutionary theorists.

Core-set terminology enables description of the response of the broader scientific community to the early work of the synthesis authors. In the initial phase the scientific community are interested on-lookers, who follow the debate and wait for the core-set scientists to negotiate the controversial issue.<sup>430</sup> These individuals are the consumers of the first wave of texts produced by the core-set (i.e. Dobzhansky, 1937; Huxley, 1942; Mayr, 1942; Simpson, 1944). During the formation of the CCP we see the core-set grow as further key participants are recruited from this body of on-lookers. This phase of recruitment represents the first change of scale of the synthesis programme. This increase in scale conferred legitimacy and increased cultural authority; the core-set's initially small esoteric circle had been expanded by subscription. This activity is anticipated in core-set situations and follows the pattern predicted in Collins' Empirical Programme of Relativism (EPOR).<sup>431</sup>

Phase one of Collins' EPOR examines the processes whereby the core-set presents new work to the broader community. This new work represents the 'interpretive flexibility of scientific knowledge' and shows that 'different interpretations of the natural world are available to different scientific actors'.<sup>432</sup> Phase two of the EPOR seeks to explain the processes by which the community considers how this new work might be integrated with existing theory, and considers how any contentious element of that work might be resolved by use of closure tactics. The architects' canonical works, and the setting up of the CCP in 1943, exemplify these two phases of activity respectively. Collins' EPOR describes how, during phase two, the mainstream community effectively decides whether new information/interpretations will be rejected or accepted. Interestingly, in the case of the Darwinian synthesis, the body set up to carry out those phase 2 tasks was largely comprised of the very authors of the new approach – a sure tactic to achieve acceptance.

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<sup>430</sup> Pinch, T. (1990)

<sup>431</sup> Collins, H. (1981a); Collins, H. (1983) An empirical relativist program in the sociology of scientific knowledge. In: K. Knorr-Cetina & M. Mulkay (Eds.) *Science observed: perspectives on the social study of science*. Sage publications, London.

<sup>432</sup> Pinch, T. (1990) p.92

Phase three considers the process by which acceptance of contentious material results in broad uptake by the mainstream scientific community. In Collins' model this final stage also takes into account the cultural context of the scientific community, which in part informs the likelihood of any such uptake. Collins suggests we should account for the impact of the wider cultural context on those processes by which consensus is reached within the scientific community, which I move on to do below.<sup>433</sup> In section 3.1.3 I describe how the synthesis authors achieved this phase of the EPOR.

In the case of the synthesis movement, the application of the EPOR reveals the fascinating way in which the synthesis authors achieved increased authority. Choices concerning the achievement of consensus, and the integration of new interpretations of the natural world, might be expected to rest with the mainstream, since this is the perceived site of intellectual power and cultural authority. However, in the case of the synthesis group, their gradual increase in scale and power allowed their activity to subvert this standard centre versus periphery expectation. The original core-set had moved closer to the centre by merit of gradual subscription from the mainstream. The final act of integration was affected not by the choice of the broader community, but rather by the self-redefinition of the group. In 1946, the group renamed themselves the Society for the Study of Evolution<sup>434</sup>, and through this device not only integrated themselves into the mainstream, but in fact redefined themselves *as* the mainstream. Thanks to their union of individually powerful supporters, and funding from the American Philosophical Society, the original core-set and their new supporters had affected an intellectual coup.

The means by which this transition was achieved is clarified if we consider the core-set in light of actor-network theory.<sup>435</sup> Mike Michael and Lynda Birke have described how scientists engage actively in constructing the core-set (and thus a

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<sup>433</sup> Pinch, T. (1990)

<sup>434</sup> Jepsen, G., Simpson, G. & Mayr, E. (Eds.) (1949)

<sup>435</sup> Actor-Network theory was largely developed as a result of: Callon, M. (1986) Some elements of a sociology of translation: domestication of the scallops and fishermen of St. Brieuc Bay. In: J. Law (Ed.) *Power, action and belief*. Routledge and Kegan Paul, London.; Latour, B. (1987) and Law, J. (1987) Technology and heterogenous engineering: the case of the Portuguese expansion. In: W. Bijker, T. Hughes & T. Pinch (Eds.) *The construction of technological systems: new directions in the sociology and history of technology*. MIT Press, Cambridge, Massachusetts.

network), by enrolling certain actors over others, in service to their agenda.<sup>436</sup> Those actors can be human or non-human, and they can be excluded from or included in a network in order to serve certain purposes. Michael and Birke suggest that scientists must interpose themselves between the target entity and its pre-existing associations, thus creating it anew as an actor in each engagement.

This process of enrolling entities is an ideal way to view the synthesis architects' actions as core-set members. In that case, they enrolled an image of Lamarck and an image of Darwin and Darwinism as actors attached to their core-set. They created those images from scratch, stripping them of their association to a protracted, complex and less-polarised history. They also enrolled the grouping tools of societies, and later the 'public' (during the centennial, as I describe below). Their construction of those actors served their interest of creating a stronger and larger network. Michael and Birke suggest that a stable network is one in which all the actors are managed effectively to contribute to a goal. In the case of the synthesis authors, and their management of the actors they enrolled, we see an excellent example of that kind of management to achieve a goal.

The next section seeks the mode of deployment of the intellectual and ideological tenets that had been set in place during the formulation of the modern synthesis, tracing the extension of Darwinian intellectual authority through cultural enforcement, network extension and phase 3 of Collins' EPOR.

### 3.1.3: The deployment of the synthetic approach and Darwinian triumphalism: boundary work at the Darwin Centennial

Sections 3.1.1 and 3.1.2 describe how the advocates of Darwinism progressively won authority in the intellectual context during the 1940s. By the 1950s their original minority endeavour had come to represent mainstream evolutionary biology. This section describes how the group affected a further dramatic increase of scale, extending their influence beyond the scientific community. In this section, the period of the Darwinian centennial (1958/1959) is discussed to illuminate how the synthesis group won cultural authority and

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<sup>436</sup> Michael, M. & Birke, L. (1994) Enrolling the core-set: the case of the animal experimentation controversy. *Social Studies of Science*, 24(1): 81-95.

completed their boundary work objectives. This section describes how cultural authority validated the old map of Darwinism.

The Darwin centennial in 1959<sup>437</sup> served several functions for the architects of the modern synthesis. The provident timing of this anniversary provided them with an excellent opportunity to affect a timely assertion, extension and deployment of the authority they had gained since the late 1930s. The notion of Darwinian triumph had been used as a key rhetorical device in the extension of authority during the 1940s. The centennial celebration provided an opportunity for these authors to reinforce and further disseminate the triumphant identity of Darwin and Darwinism.

The centennial celebration provided the opportunity to continue cohering and extending the intellectual programme of the synthesis gang. Opportunities to physically regroup and discuss new developments were important for the advancement of the modern synthetic approach. In addition, the more informal, less intellectually charged element of the centennial meetings, i.e. the celebration by non-professionals of Darwin as a scientific hero, provided a promotional opportunity.

The University of Chicago Darwin Centennial Celebration (November 1959) provides the best example of how this event was commandeered by certain individuals, institutions and communities to serve specific functions and suit various agendas. That celebration manifest boundary work with two functions: First, the coherence, reinforcement and extension of the intellectual authority of Darwinian evolutionary theory. Second, the extension of cultural authority, such that Darwinism would attain further authority in relation to other adjacent areas of expertise and would achieve authority in the public awareness.<sup>438</sup>

The University of Chicago began planning its centennial in 1955, with resident anthropologist Sol Tax as chief organiser.<sup>439</sup> Tax was assisted in his

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<sup>437</sup> In this discussion I refer to 1959 as the year of the Darwin centennial. This is based upon the selection of that year (100 years since the publication of the *Origin of Species*) for the celebration at Chicago University. It should be noted that other groups, notably many in the UK, chose to celebrate the centennial in 1958 to mark one hundred years since the reading of Darwin and Wallace's joint paper on natural selection at the Royal Society [Smocovitis, V. (1999) The 1959 Darwin centennial celebration in America. *Osiris*, 2<sup>nd</sup> Series, 14: 274-323]

<sup>438</sup> Asserting authority to those outside the immediate intellectual community was important. Territories are agreed, and cultural authority is allocated, through negotiation with those who share your boundaries. It is no use to say that you have all the authority if nobody agrees.

<sup>439</sup> Smocovitis, V. (1999)

planning by key members of the Society for the Study of Evolution.<sup>440</sup> In fact, the role of the SSE in planning and contributing to so many of the worldwide centennial celebrations was so marked that its president, Edgar Anderson, began to suffer stress and exhaustion related illness. Upon finally attending the Chicago Celebration in 1959, after so much hard work in the planning phase, Anderson reported that the event and its speakers ‘got him so excited that they eventually ‘did him in’.<sup>441</sup> Anderson was subsequently hospitalised for three weeks.<sup>442</sup> The synthesis supporters had again positioned themselves to maximise their influence. Their heavy involvement with the organisation of the celebrations meant that their agenda was carried forward and deployed in new contexts, just as it had been when the CCP and SSE were established.

One of the earliest Chicago centennial organisational tasks, carried out in 1956, was the recruitment of about fifty scientists as contributors to the planned centennial event.<sup>443</sup> A series of themes for discussion were agreed upon and during 1957 and 1958 the chosen authors individually prepared papers that would later be collected as a published report of the meeting. For the meeting itself, the authors would split into groups and hold topic based panel discussions with the public.<sup>444</sup> The aim overall appears to have been to effect a ‘live’ version of the task that Dobzhansky had begun in 1937 when he published *Genetics and the Origin of Species*; to subject issues related to evolution to the analytical and descriptive power of Darwinism.

The conference began on November 24<sup>th</sup>, 1959 and ended November 28<sup>th</sup> in time for the Thanksgiving holiday. At least 2,500 people registered to attend, along with the 250 delegates, representing 189 colleges, societies and institutes who presented material at the event.<sup>445</sup> Historian Vassiliki Smocovitis describes this event as: ‘five days of scientific discussions, pageantry, ritual and theatrical spectacle’.<sup>446</sup>

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<sup>440</sup> Tax describes the organization of the Chicago centennial in: Tax, S. (1960a) Introduction. In: S. Tax (Ed.) *Evolution after Darwin, Volume 1: the evolution of life*. University of Chicago Press, Chicago.

<sup>441</sup> Smocovitis, V. (1999) p. 277

<sup>442</sup> Smocovitis, V. (1999)

<sup>443</sup> Tax, S. (1960a)

<sup>444</sup> Tax, S. (1960a)

<sup>445</sup> Smocovitis, V. (1999)

<sup>446</sup> Smocovitis, V. (1999) p. 278



What was the boundary work function of the centennial celebrations, and the Chicago meeting in particular? How did the centennial help extend the cultural authority of Darwinism? Smocovitis suggests numerous means by which celebrations of this type can be manipulated to serve such agendas within science. Several of the functions she identifies in the case of the Chicago Darwin Centennial are relevant to this discussion.

Firstly, Smocovitis identifies the apt timing of the Darwin centennial as a particular benefit to the authors of the modern synthesis at this juncture in their rise to authority. By the late 1940s the synthesis architects had established their endeavour as a 'new discipline with a self-aware community of individuals who identified themselves as 'evolutionary biologists''.<sup>447</sup> Thus, they had attained authority within the intellectual and academic community. The timing of the centennial provided them with an opportunity to consolidate and extend that influence. As Smocovitis states:

'The anniversary of the publication of the landmark work ushering in their field of interest was a well-timed opportunity to retell the life story of Charles Darwin, who was reinvented as the 'founding father' of their discipline. It also served as the perfect opportunity to establish once and for all - for wide audiences - the facility of evolution by natural selection.'<sup>448</sup>

A retelling of the history of Darwinism, particularly with the triumphant tone that this celebration inspired, was of vital importance to the synthesis group since '...so much of what it meant to be a twentieth-century evolutionary biologist hinged on identification with the narrative of Darwin's life and work'.<sup>449</sup> To the non-professional audience attracted by the centennial events the assertion of a triumphant Darwinian narrative, as a vindication and affirmation of the synthesis work, acted as a powerful tool for recruiting support. The story of a 'founding father' acted with a public relations service; history was being used to compound the identity and authority of the new 'evolutionary biologists'. Through the retelling of the narrative

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<sup>447</sup> Smocovitis, V. (1999) p. 279

<sup>448</sup> Smocovitis, V. (1999) p. 279

<sup>449</sup> Smocovitis, V. (1999) p.279

of Darwin's life and work he was constructed as both an icon of the 'modern' and of unified biology, and also as a cultural symbol of success and even genius in the science. History served boundary work; affirming the status and authority of Darwinism and suggesting that a legacy of 'genius' and triumph underlay that authority. The history of Charles Darwin's own life and work was used to convey this sense to the public.

Historian Patricia Fara has described the process of the construction of scientific genius in relation to Issac Newton.<sup>450</sup> Fara uses the case study of the construction of Newton's genius in scientific and popular culture as a means by which to identify the traits of an individual that lend them to this treatment. The categories Fara identifies provide a useful tool for understanding the process by which Charles Darwin's character and enterprise were reconstructed during the formulation of the modern synthesis and the centennial celebrations. Using her model we can explain the construction of Darwin's genius as boundary work purposed to extend the authority of Darwinism in the cultural domain. In particular, Julian Huxley's contribution to the three-volume report of the Chicago Centennial provides a clear illustration of that project.<sup>451</sup>

Fara says that the traditional 'genius' is generally set apart from society.<sup>452</sup> In the retelling of Darwin's life story during the centennial this theme was highlighted. Darwin's isolation aboard the HMS Beagle, his self imposed seclusion in later life at his remote home at Downe House, the loneliness of his prolonged illnesses and the isolating guilt concerning his 'secret' research and theory all became themes.<sup>453</sup> This image of genius often involves moments of epiphany or vision. In the case of Newton we are presented with the iconic image of his seeing a falling apple and identifying the force of gravity. The idea of moments of imagination is key to the traditional picture of genius, since it strips the subject from the context of a scientific community making them isolated in their endeavour. Julian Huxley's centennial contribution highlights this theme in relation to Darwin, stating that:

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<sup>450</sup> Fara, P. (2002) *Newton: the making of genius*. Columbia University Press, New York.

<sup>451</sup> See Huxley, J. (1960) The emergence of Darwinism. In: S. Tax (Ed.) *Evolution after Darwin, Volume 1: the evolution of life*. University of Chicago Press, Chicago.

<sup>452</sup> Fara, P. (2002)

<sup>453</sup> Huxley, J. (1960) and Barlow, N. (Ed.) *The autobiography of Charles Darwin*. Collins, London.

‘...late in 1938 he ‘happened to read for amusement *Malthus on Population*’ ...and the idea of natural selection immediately flashed upon him.’<sup>454</sup>

In addition, he emphasises the role of imagination in Darwin’s work, stating that:

‘Although his laborious patience in the collection and synthesis of factual evidence has rarely been rivalled...yet sudden intuition was responsible for some of his most important discoveries of principle, notably natural selection and the explanation of biological divergence...’<sup>455</sup>

Also, Fara states that a genius must manifest an internal creative urge and passion.<sup>456</sup>

Again, Darwin’s life story supports this requirement. We hear how Darwin strived to develop his theory over many years, not wanting to publish until he had perfected the work. He was tormented by the theological implications of his work, and yet was driven by an urge to produce a theory. In the Chicago Centennial publications Julian Huxley says of Darwin that: ‘he had an inborn passion for natural history...’ and that:

‘Another characteristic of Darwin was his extraordinary diffidence, coupled with a passion for completeness and a reluctance, so extreme as to appear almost pathological, to publish to the world his ideas on the controversial subject of evolution before he had buttressed his arguments with a body of evidence which would overwhelm opposition by its sheer vastness.’<sup>457</sup>

Huxley also speaks of Darwin’s ‘constant ill health’ and ‘neurotic symptoms springing from unconscious conflict or emotional tension’.<sup>458</sup> Reference to these aspects of Darwin’s personality reinforces the notion of his passion; his dedication was so profound as to permeate his mental state and cause his health to deteriorate.

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<sup>454</sup> Huxley, J. (1960) p.5

<sup>455</sup> Huxley, J. (1960) p.5

<sup>456</sup> Fara, P. (2002)

<sup>457</sup> Huxley, J. (1960) p.2-3

<sup>458</sup> Huxley, J. (1960) p.3

Attention to these details also fosters the Romantic image of Darwin as a man 'tortured' by his art, a popular theme in the creation of images of genius.

Fara describes the traditional genius as an individual constructed almost as a religious icon, or whose work appears to have the presence of religious ideology.<sup>459</sup> The story of Darwin's endeavour lends itself to this construction. Darwin himself was well aware of the conflict of his theory with religious doctrine. His work was received as a challenge to established theology. So, rhetoric arises defining Darwin as a force in the progressive secularisation of society. Darwin's theory strips God from nature and by that act his work replaces theology. In the Chicago centennial publication, Huxley highlights this aspect of Darwin's endeavour, quoting from a 1844 letter (from Darwin to Hooker) Darwin's confession that: 'to assert that species are not immutable is 'like confessing to a murder'!'.<sup>460</sup> Huxley emphasises Darwin's guilt, using the evidence of his delay of publication to highlight the conflict between his work and theology. Not only does this imply that Darwin's work took on a significance akin to religious doctrine as it became accepted in the twentieth century, but also this rhetoric serves the additional function of presenting Darwin as a true revolutionary in a time ill-prepared for his contribution.

In addition to these inferences of scientific genius there are some more insistent statements on Darwin's genius. For example, in the Chicago Centennial publication, Julian Huxley says of Darwin's contribution to evolutionary work that: '...he also contributed far more than Wallace, or indeed than any other man, to the solution of the problem and the development of the subject.'<sup>461</sup> He goes on to say: 'Charles Darwin has rightly been described as the 'Newton of Biology'...'.<sup>462</sup>

Huxley also indicates that even as he wrote this account of Darwin's life for the centennial collection he was mindful of a programme for the construction of genius and was self-aware of his role as an agent of that construction. He refers to Kroeber, who had written on the anatomy of genius, stating that:

'Kroeber has demonstrated that the effective manifestation of genius requires not

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<sup>459</sup> Fara, P. (2002)

<sup>460</sup> Huxley, J. (1960) p.4

<sup>461</sup> Huxley, J. (1960) p.1

<sup>462</sup> Huxley, J. (1960) p.1

only an exceptional individual talent, but depends also on the circumstances and sometimes accidents of place and period; nowhere is this better illustrated than in the person of Darwin.’<sup>463</sup>

So, as the centennial celebration enlivened the history of evolutionary thought the public were being taught that Charles Darwin and evolutionary theory were synonymous. The focus on particular features of Darwin’s personality and work played to the cultural identity of ‘genius’, facilitating the construction of Darwin as a historical icon. The centennial materials put history to work to validate the authority of twentieth century Darwinian theory. One key result was that, as the centennial celebration boosted Darwin’s cultural identity, the authors of the modern synthesis garnered public support in respect of their role as Darwin’s contemporary manifestation.

As well as increasing the cultural authority of Darwinism, the centennial celebration also played a role in internally strengthening the ‘synthesis gang’. Support for the synthetic approach from within the academic community had increased enormously in the late 1940s, particularly in the wake of the foundation of the SSE and the establishment of the journal *Evolution*. After this rapid phase of recruitment the nature of the programme (as advised in Dobzhansky (1937) and Huxley (1942)) was in need of reinforcement, and its protagonists and newer supporters required an opportunity to take stock and consider their new authority. Smocovitis identifies the regrouping function of celebration as a vital motivation for the organisation of large-scale events like the centennial.<sup>464</sup> She describes how this event was nested with the development of the synthesis that had occurred in the previous two decades:

‘In the wake of the evolutionary synthesis of the 1930s and 1940s, the anniversary in 1959, coming twelve years after the Princeton meetings (during which evolutionists celebrated the reconfiguration of the biological disciplines around the new science of evolutionary biology), was perfectly timed to reassess the state of the art by the

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<sup>463</sup> Huxley, J. (1960) p.8

<sup>464</sup> Smocovitis, V. (1999)

community of individuals that had worked to create a synthetic, unified science of evolution.<sup>465</sup>

During the Chicago Centennial the voices of the early architects of synthesis returned to the fore in what had become, by that time, a much larger community. This act, of what Smocovitis would call ‘disciplinary memory’<sup>466</sup>, allowed their agenda to regain any influence that had been lost in the larger/more diffuse endeavour that evolutionary biology had become. By invoking ‘disciplinary memory’ the identity of the group was reinforced, and the synthetic approach was solidified around the core ideals that had inspired it. In volume one of the conference proceedings, we see the strong alliance of the original core-set reunited at the forefront of evolutionary biology. Papers by Dobzhansky, Huxley, Simpson and Mayr all appear in this volume.<sup>467</sup>

The Chicago centennial was only one of a huge number of celebrations undertaken internationally. The functions of celebration identified by Smocovitis can be viewed as generally applicable across that mass of celebrations. The international celebration of the Darwin centennial had a considerable impact, a key element of which was to publicise the intellectual material of the modern synthesis to the public. Through the action of celebration, manifest both in prolific new publications and actual physical celebratory events, the modern synthesis and Darwinian triumphalism were given new cultural and social meaning through communication to new audiences and broader availability. The primary tools used for this communication were the reiteration of the history of the rise of Darwinian theory and the iconisation of Charles Darwin himself. These two devices formed the boundary work by which this next phase of monopolization was achieved.

#### 3.1.4: The boundary function of the centennial publications: publication as a means of programme extension and deployment

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<sup>465</sup> Smocovitis, V. (1999) p.278

<sup>466</sup> Smocovitis, V. (1999) identifies the act of invoking ‘disciplinary memory’ as one of the key tools in the process of defining disciplinary identity. This occurred on two levels at the time of the centennial celebrations. Firstly, the community invoked the memory of Darwin, as a founding father of evolutionary theory. Secondly, the community of evolutionary theorists invoked the memory of its own ‘founding fathers’ (i.e. Dobzhansky and Huxley) as a way of asserting their identity.

<sup>467</sup> Tax, S. (Ed.) (1960a)

A brief overview of some of the materials published in the period around the 1958/1959 Darwin centennial demonstrates the next stage of boundary work for expansion and the drive towards monopoly. The texts are too numerous to discuss here in specific detail. Therefore, this section seeks to highlight categories within that body of material, assessing their role in the process of extension and the deployment of the synthetic programme in the wider cultural context. Broadly speaking the published centennial materials fall into four main categories.

Firstly, there are materials that represent the ongoing intellectual work of the synthesis architects and their increasing network of supporters. These materials address epistemological and theoretical problems, and seek to extend the application of Darwinism to new kinds of intellectual problems in other areas of biology. They rely upon the approach that had been applied during the early years of synthesis. Their boundary work devices are the same; the assertion of Darwinism as the principal tool for biology and the concurrent removal of Lamarckian alternatives. In some cases, these materials represent the outcomes of physical meetings or symposia organized as special centennial events by various organisations or groups. One of the most notable works of this type is the three-volume Chicago centennial series edited by anthropologist Sol Tax.<sup>468</sup> This work features contributions from the principal architects of the synthetic movement, and summarises the intellectual progress made during the panel sessions held at the Chicago meeting. Analogous materials were also generated from centennial meetings at other locations. For example, extensive proceedings were produced by the Botanical Society of the British Isles<sup>469</sup>, The Royal Society of Victoria, Australia<sup>470</sup> and from the Centenary and Bicentenary Congress held in Singapore<sup>471</sup> amongst others. This category also includes new

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<sup>468</sup> Tax, S. (1960a); Tax, S. (Ed.) (1960b) *Evolution after Darwin, volume 2: The evolution of Man*. Chicago University Press, Chicago; Tax, S. (Ed.) (1960c) *Evolution after Darwin, volume 3: Issues in evolution*. Chicago University Press, Chicago.

<sup>469</sup> See Wanstall, P. (Ed.) (1961) *A Darwin Centenary*. Botanical Society of the British Isles, London. The report of the conference held by the Botanical Society of the British Isles in 1959 to mark the publication of *The Origin of Species*.

<sup>470</sup> Leeper, G. (Ed.) (1962) *The Evolution of Living Organisms*. Melbourne University Press, Parkville. The report of a symposium to mark the centenary of Darwin's *Origin of Species* and of the Royal Society of Victoria, held in Melbourne, December, 1959.

<sup>471</sup> *Centenary and Bicentenary*. The report of a congress held in Singapore from December 2-9, 1958, in celebration of the centenary of the formulation of the theory of evolution by Charles Darwin and Alfred Russell Wallace, and the bicentenary of the publication of the 10<sup>th</sup> edition of the *Systema*

academic texts produced without a connection to a physical meeting or celebration. Some examples of this type of work are *Nature and Man's Fate: On Evolutionary Theory and Human Evolution, With Special Reference to Darwin's 'Origin of Species'*,<sup>472</sup> which extended the application of Darwinism to the circumstances of human evolution, and *Darwin and the Modern World View*.<sup>473</sup>

Secondly, there are materials that promote the cohesion of the modern synthetic programme and its supporters. These materials had two functions: First they acted to internally strengthen the programme through the act of 'disciplinary memory'. Second, they made key materials of the synthetic movement available to new and diverse audiences. These texts were of several types. For example, in 1964 Dobzhansky's *Genetics and the Origin of Species* was reissued<sup>474</sup>, which can be seen as both an act of disciplinary memory for the synthesis authors and a reassertion of his original synthetic programme. Also, the 1958 publication of *A Book That Shook the World: Anniversary Essays on Charles Darwin's 'Origin of Species'*<sup>475</sup> brought together the early core-set members, Dobzhansky and Huxley (amongst others), as contributors to a text summarising the impact of Darwinism on contemporary biology. Furthermore, in 1964, a facsimile version of the first edition of Darwin's *Origin of Species*, with a new introduction by Ernst Mayr was published.<sup>476</sup> This edition of Darwin's text was the favoured version of the agents of the modern synthesis, since it expressed Darwinian theory in its original and 'purest' form; that is the form that Darwin presented prior to later amendments when he strayed further towards Lamarckian explanations for hereditary.<sup>477</sup> It is this first edition that asserts natural selection in its strongest form, and therefore the edition that most suited the synthesis authors as a reference for their Darwinism. The reissuing of this text made

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*Naturae* by C. von Linnaeus.

<sup>472</sup> Hardin, G. (1959) *Nature and Man's fate*. Rinehart, New York.

<sup>473</sup> Greene, J. (1961) *Darwin and the modern world view*. Louisiana State University Press, Louisiana.

<sup>474</sup> Dobzhansky, T. (1964) *Genetics and the origin of species*. (3<sup>rd</sup> edition, revised) Columbia University Press, New York.

<sup>475</sup> Huxley, J. (Ed.) (1958) *A book that shook the world: anniversary essays on Charles Darwin's origin of species*. University of Pittsburg. Pennsylvania.

<sup>476</sup> Mayr, E. (Ed.) (1964) *The origin of species: facsimile of the first edition*. Harvard University Press, Massachusetts.

<sup>477</sup> Huxley had tried to limit the damage of Darwin's own Lamarckism to the story of Darwin as the founding father of modern evolutionary biology. In his centennial contribution he stated of Darwin's Lamarckian references that: 'These 'Lamarckian' errors clearly sprang from the total ignorance of 19<sup>th</sup> century biology on the subject of heredity'. [Huxley, J. (1960) p.14] Releasing Darwin from responsibility for Lamarckian sympathies.



their favoured primary source available to new audiences.

Thirdly, there are materials generated purely for the purpose of communicating synthetic Darwinian theory to a wider, generally non-specialist audience. These texts represent some of the earliest ‘popular’ accounts of Darwin and Darwinism. Key examples in this category include the production of historical and biographical materials on Darwin and Darwinism, and the production of guides to the reading of the primary sources for the non-specialist. Several biographies were issued in this period for example *Charles Darwin*<sup>478</sup>, *Charles Darwin: a great life in brief*<sup>479</sup>, *Charles Darwin: evolution by natural selection*<sup>480</sup> and *Charles Darwin the Founder of the Theory of Evolution and Natural Selection*.<sup>481</sup> Also, a version of Darwin’s autobiography was produced in 1958 with Darwin’s granddaughter, Nora Barlow, as editor.<sup>482</sup> This text was particularly valuable as it was short and easy to approach for the non-scientist. 1958 also saw the publication of the same autobiography alongside selected letters by Francis Darwin.<sup>483</sup> The production of these texts made ‘life and times’ material on Darwin available to the more general reader, which was essential to the successful construction of Darwin as the founding father of modern evolutionary thought. The role of Darwin as founder was also reinforced by the publication of historical works addressing the impact of his contribution, for example, Charles Darlington’s *Darwin’s Place in History*.<sup>484</sup> Non-specialist access to the material of Darwinian theory was also facilitated in this period by texts such as *The Darwin Reader*, which was published one year apart in both America and Britain.<sup>485</sup>

Finally, there are publications that served the function of increasing access to sources on Darwin and Darwinism. This category includes the numerous foreign language translations of existing materials that were produced as an outcome of the centennial. These include translations of *The Origin of Species*, along with various

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<sup>478</sup> First UK edition: Moore, R. (1957) *Charles Darwin* Hutchinson, London.

<sup>479</sup> First US edition: Moore, R. (1955) *Charles Darwin: a great life in brief*. Alfred Knopf, New York.

<sup>480</sup> De Beer, G. (1963) *Charles Darwin: evolution by natural selection*. Nelson, London.

<sup>481</sup> Wichler, G. (1961) *Charles Darwin: the founder of the theory of evolution and natural selection*. Pergamon, Oxford.

<sup>482</sup> Barlow, N. (Ed.) (1958) *The autobiography of Charles Darwin*. Collins, London.

<sup>483</sup> Darwin, F. (Ed.) (1958) (reprint from 1892, 1st edition) *The autobiography of Charles Darwin*. Dover Publications, New York.

<sup>484</sup> Darlington, C. (1959) *Darwin’s place in history*. Basil Blackwell, Oxford.

<sup>485</sup> Bates, M. & Humphrey, P. (Eds.) (1956) *The Darwin reader*. Charles Scribner’s Sons, New York; Bates, M. & Humphrey, P. (Eds.) (1957) *The Darwin reader*. Macmillan, London.

other specialist and non-specialist texts, in various European languages, for example, French<sup>486</sup>, German<sup>487</sup>, Danish<sup>488</sup>, and Portuguese<sup>489</sup>.

The huge wealth of material published as part of the centennial celebrations served to increase the appearance that the Darwinism was the principal, if not the only, tool for evolutionary work. By the mid-1960s the key primary sources of the Darwinian movement were readily available, and there was an abundance of material relating to Darwin and Darwinism for the wider audience. Partly on account of these materials, and partly on account of the high profile of the centennial celebrations themselves, Darwinism had been given a new and clear meaning to a hugely extended audience nationally and internationally. Darwin's name had become synonymous with the concept of evolution. The synthesis architects had first implied monopoly (i.e. in the naming of the SSE), but as they captured the public imagination and regrouped their endeavour during the centennial they achieved the monopoly that they sought. By the mid-1960s evolutionary biology was monopolized by the synthetic approach. And that achievement was not transient. An enduring identity of Darwinism had been created. Mayr has remarked that: '...no major revision of the consensus of the 1940s became necessary during the ensuing years, only a somewhat increased sophistication in certain formulations that had been oversimplified originally'.<sup>490,491</sup> The consensus on Darwinian monopoly within biology was significant. Mayr suggests that by 1947 those scientists who had not become Neo-Darwinians were simply the remains of an 'older generation' who were 'unable to convert'.<sup>492</sup>

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<sup>486</sup> For example: Canguilhem, G. (1959) Les Concepts de 'lutte pour l'existence' et de 'selection naturelle' en 1858: Charles Darwin et Alfred Russel Wallace. *Les Conférences du Palais de la Decouverte*, Series D: histoire des sciences, 61: 17.

<sup>487</sup> For example: Genschel (1959) *Charles Darwin: Mensch zwischen Glauben und Wissen*.

<sup>488</sup> For example: Ostenfeld (1959) *Charles Darwin: Personlighed og sygelighed*.

<sup>489</sup> For example: *Seleção natural. Centenário da comunicação de Darwin and Wallace sobre a seleção natural*. Publicações aculsas do Museu Nacional No. 46, 1963.

<sup>490</sup> Mayr, E. (1980a) p.43

<sup>491</sup> That oversimplification can be seen as a legacy of Dobzhansky's recommendation that treatments be 'assertive' rather than 'polemic'. [See: Dobzhansky, T (1937) Preface] He intended that style as a strategy for the promotion of Darwinism. Once a broad Darwinian consensus had been achieved less assertive texts were an option and that return to greater detail and 'sophistication' is essentially what Mayr has noted in the period post-1940s.

<sup>492</sup> Mayr, E. (1980a) p.43

The next phase of increase in the cultural authority of Darwinism came when other kinds of authors and specialists turned attention to the theory and its development. The high profile of the centennial publications made them a perfect source for historians and for textbook writers in the period after 1960. Also, the journal *Evolution* made some of the more technical work of the synthesis readily available to authors of secondary sources on Darwin and Darwinism. Any historian of science or textbook writer turning attention to evolutionary theory in this period would find abundant information on the rise of the modern synthesis and its relation to Darwin's nineteenth century work. The nature of the materials available gave the impression that Darwinian triumph characterised the history of modern evolutionary theory; the volume and high profile of those materials would create the appearance of a single-track history for the development of evolutionary thought.

The broad uptake of this triumphalist narrative is evident in the decades that follow the Darwin centennial. For example, evolutionary theory began to be transmitted to new generations through school textbooks, with the focus on Darwin as founding father. Elements of Charles Darwin's biography were attributed iconic status as part of the pedagogical construction. In particular, Darwin's study of the Galapagos finches became a popular tool for introducing the concepts of variation and interspecies competition that are foundational to an understanding of natural selection.<sup>493</sup> As the new generation of biologists was trained they learned that Darwinism was synonymous with evolutionary theory. By 1980 Mayr remarked that: 'The Darwinian ...interpretation of evolution is now so nearly universally accepted among biologists that the present generation of evolutionists can hardly comprehend the opposition that the theory of natural selection still encountered in the 1920s and 1930s.'<sup>494</sup>

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<sup>493</sup> For example, these trends are illustrated in the materials of the Nuffield Foundation Teaching Project, created between the 1960s and 1980s. The Nuffield Project was the most significant attempt to control and guide science teaching in the UK prior to the 1988 introduction of the National Curriculum. The foundation provided comprehensive teaching materials and textbooks, thus carrying their agendas and perspectives into classrooms nationwide. The Nuffield Biology Series reflects the triumphalist narrative of Darwinism, and teaches the modern synthesis exclusively as the theory of evolution. I have studied the Nuffield reforms and materials in detail elsewhere. See: Jarvis, L. (2000) *Science in textbooks: A study of the Nuffield Foundation Science Education Reforms, 1960-1988*. MSc thesis, Imperial College London.

<sup>494</sup> Mayr, E. (1980a) p.3

In the post-1960 deployment of Darwinian authority, Ernst Mayr took another significant role as he transformed himself into a historian of Darwinian theory. In the decades after the centennial, Mayr's publications host his deployment of the triumphalist narrative. It is perhaps in his work that we see one of the best examples of the preservation of the old map record of Darwinism. For example, Mayr's 1976 *Evolution and the diversity of life* is a collection of his own essays, which have been compiled in such a way as to reinforce the goals of the synthesis group. Mayr follows the principles of the early synthesis authors: i) he restates the theory of evolution, which is represented only by Darwinian theory ii) he asserts the broad value of the theory as a tool for unification in biology and iii) he dismisses the opposition of Lamarckism. In that text Mayr achieves that third goal by means of an interesting rhetorical device. Rather than ignoring Lamarckism Mayr states that he will address it, but qualifies that attention by saying that:

'As long as the battle between Darwinians and Lamarckians was raging, it was quite impossible to undertake an unbiased evaluation of Lamarck. For this we are now ready, after it has been demonstrated conclusively that the various causal explanations of evolution, usually designated as Lamarckism, are not valid. Not that it really needed this final proof...' <sup>495</sup>

Mayr is not arguing against Lamarckism, nor is he ignoring it, instead he sets out to describe it as a historical phenomenon - a vanquished opponent to Darwinism. Again, implied authority is being used as a boundary work device. In case any doubt remained Mayr's conclusion on the issue of Darwinism versus Lamarckism is that:

'This fight is now a matter of history since the Darwinian interpretation of the causal explanation of evolution has gained a total victory; it is now accepted by every well informed biologist.' <sup>496</sup>

Other authors joined Mayr in the deployment of the triumphalist narrative. Andrew Brown has noted that self-styled ultra-Darwinian Richard Dawkins

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<sup>495</sup> Mayr, E. (1976) *Evolution and the diversity of life*. Harvard University Press, Massachusetts. p.222

<sup>496</sup> Mayr, E. (1976) p.248

proclaimed, in his 1976 best-seller *The Selfish Gene*, that: ‘There is such a thing as being just plain wrong, and that is what, before 1859, all answers to these [evolutionary] questions were.’ And that, in 1995, Daniel Dennett added that ‘natural selection is a ‘universal acid’ able to eat away at all other explanations of what goes on in the world until the true Darwinian stories of competition among genes appear.’<sup>497</sup> Brown suggests that the result is that modern Darwinism seems ‘intoxicatingly powerful’.<sup>498</sup> These kinds of statements are all the more important to the deployment of the triumphalist narrative because they appear in popular works, intended for mass consumption by non-scientific readers. These authors are involved, alongside many others, in policing the boundaries of the Darwinian authority that had been established by the twentieth century boundary work.

The result of all this remains culturally manifest today. Charles Darwin remains synonymous with evolution in popular culture, and his iconic status continues to be fostered by the media and advertisers who use his image and ideas frequently in parody, pun and paraphrase. In addition, educational textbooks still echo the Darwin triumphalism so keenly emphasised during the centennial, a situation that Michael Ghiselin attributes as a function of the ‘successive teams of plagiarists’ that create these kinds of texts.<sup>499</sup> Zoologist Mark Pagel has observed that: ‘Darwin’s ideas have been caricatured, misrepresented, used and abused’ and that ‘politicians, schoolteachers, academics, doctors, lawyers, economists, psychologists and even dieticians use him for their ends’.<sup>500</sup> All this elevates Darwinism to a position of high status in cultural awareness.

In the case of the history of science, the circumstances of Darwin’s depiction have changed to a degree on account of epistemological transitions within that discipline. Between the 1950s and 1970s, as the history of science emerged as an independent specialism, much attention was directed toward the creation of scientific biographies. The focus of these works were those scientists who might be considered as ‘founding fathers’ for modern science. This type of work was necessarily present-

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<sup>497</sup> Brown, A. (1997) Feud for thought. *The Guardian*, 11.06.97 [cf. Dawkins, R. (1976) *The Selfish Gene*. Oxford University Press, Oxford; Dennett, D. (1995) *Darwin’s dangerous idea: evolution and the meanings of life*. Penguin, London.]

<sup>498</sup> Brown, A. (1997) p.2

<sup>499</sup> Ghiselin, M. (1994) Replicating a fiction. The imaginary Lamarck: A look at bogus ‘history’ in schoolbooks. *The Textbook Letter*, 5 (4). Online at: [www.textbookleague.org/54mark.htm](http://www.textbookleague.org/54mark.htm)

<sup>500</sup> Pagel, M. (1998) A man for all seasons. *New Scientist*, 2120: 44–45

centred and fixated upon 'genius'. As the discipline matured over the following two decades this approach began to be perceived as 'Whiggish' and too uni-dimensional to provide a realistic expose of past scientific activity. The most significant move to reform this style was precipitated by the introduction of the social constructivist approach. That approach sought to contextualise past scientific activity against a rich backdrop of contemporary social, political and cultural history. Ironically, even though the history of science took this turn away from 'the great white man of science' tradition, Charles Darwin remained a focus of study. Instead of histories that described his tortured genius, the new accounts began to ground Darwin's endeavour in the Victorian context. Although Whig history had been abandoned, Darwin remained a favourite subject for the new kinds of treatment. This is perhaps because the deployment of Darwinian triumphalist histories since the 1950s had left a legacy that was difficult to identify as a bias, and even more difficult to escape as a tacit influence.

In conclusion, this section has illustrated how the agents of the modern synthesis effected their transition to a position of significant authority and influence within the scientific community. The contest for authority began when a 'core-set' launched a bid for monopolization of evolutionary science. Through a phase of recruitment, the core-set became a community of 'evolutionary biologists'. In this transition, they carried with them a strong agenda of Darwinian advocacy coupled with disregard for Lamarckism. They utilised the affirmative power of history to provide a legacy for their Darwinian adherence, and Darwinism became the principal theory of evolution. That community, with the original core-set intact at the heart of a much larger association, then deployed the power they had gained to create a culture of Darwinian adherence both internal and external to the cultural territory 'science'. To affect this extension that community utilised the timely Darwin centennial as a tool for reassertion and deployment of their agenda. Cultural authority was then propagated under the theory's own momentum as, often subconsciously, other kinds of authors took up and transformed the material of the synthesis and the centennial to create new kinds of intellectual and cultural products.

By the time of the directed mutation debate in the 1980s and 1990s the authority of Darwin and Darwinism in relation to the cultural cartography was so marked and had such a legacy that the very notion of anti-Darwinian work was by nature controversial, inflammatory and intellectually repellent. An old map had been created and had gained persistence through the boundary acts of its architects. It had reached a level of persistence and tacit cultural significance that meant that it would be called upon in any new conflict in evolutionary theory. The immediate defence of Darwinism in response to the early directed mutation reports (identified in chapter 2) can be seen as an example of an old map being unfolded and consulted by communities convinced of the stability of Darwinian authority. The use of that old map had a profound influence on the negotiation phase of the directed mutation debate; being largely responsible for evoking the highly contentious Lamarckism versus Darwinism aspect of the debate. The mode of negotiation of directed mutation, and its perpetuation, becomes clearer when we realise that activity in that late twentieth century evolutionary conflict has its roots not in the specific or local circumstances of the debate, but rather in the historical legacy of boundary activity undertaken many decades before.

### **3.2: The ‘iconic failure’ of Lamarck and Lamarckism: A second old map influence on the directed mutation debate.**

The directed mutation debate was not only shaped by cultural and intellectual adherence to Darwinism. The Lamarckian associations of the research also invoked other areas of the old map, on which the accumulated outcomes of contests between Lamarckism and Darwinism had been recorded. The old map identity of Darwinism validated its authority, while the old map identity of Lamarckism recorded defamation, failure and rejection. In the late twentieth century the old map representation of Lamarckism was deployed to imply predetermined failure in this new contest.

John Cairns had constructed directed mutation as a possible Lamarckian phenomenon. The historical legacies of the rise of Darwinism, coupled with the historical legacies of the decline of Lamarckism, served to make that framing

particularly controversial. Section 3.1 identifies the development of positive attitudes towards Darwinism throughout the mid-late twentieth century. While this process was occurring an associated, but negative, set of attitudes was developing towards Lamarckism. As the authority of Darwin and Darwinism was being constructed, Lamarckism was being both passively and actively shaped as its antithesis.

Two factors contributed to the construction of the negative identity of Lamarckism:

- i) The notion of failed Lamarckian opposition to Darwinism was constructed as a part of the triumphalist narrative. This was achieved in two ways. First the triumph of Darwinism was implied, rather than stated explicitly, by the extraction of Lamarckism from the works of the early synthesis authors. Second, explicit reference to failed Lamarckism was useful to the Darwinian narrative since Lamarck could act as an anti-hero foil to the Darwinian hero that was being constructed. The notion of the rise and success of Darwinism was asserted and highlighted by reference to the theory's triumph over Lamarckian opposition, especially during the period of the Darwin centennial. The notion of a hero and of triumph is more meaningful where a villain or opponent is identified. For example, in the Chicago centennial publication, Huxley describes Darwin's ardour and dedicated study and suggests that this resulted in his theory being 'impressive' and 'convincing', he contrasts Darwin's theory with Lamarckism, which he characterises as a 'brief sketch' and a 'speculative picture'.<sup>501</sup> Michael Ghiselin has identified the perpetuation of this kind of comparison as part of the persistent triumphalist narrative of Darwinism in the late twentieth century. He suggests that textbook authors and other kinds of writers use reference to a constructed image of Lamarck to enhance the image of Darwinian

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<sup>501</sup> Huxley, J. (1960) p.8



success in their representations. Ghiselin argues that the result of that tactic has been the construction of a ‘false dichotomy’.<sup>502</sup>

- ii) Other factors than the rise of Darwinism also contributed to the old map identity of Lamarckism. The history of twentieth century Lamarckism includes the activity of individuals, in particular Paul Kammerer and Tofim Lysenko, who as advocates of Lamarckism are alleged to have undertaken fraudulent practice. Their stories have received attention because narratives of conflict and pathological science have broad appeal as popular historical accounts. The construction of these Lamarckian sympathisers as villains is self-sufficient and does not require the foil of a Darwinian hero to encourage interest. These individuals are constructed as ‘iconic failures’, and at this level narratives concerning their activity are complementary rather than contributory to the history of Darwinism recorded on the old map.

In this section a selection of representations of Lamarck and Lamarckism are considered to demonstrate the action of: i) the construction of a defamed history of Lamarckism with a service role to the triumphalist Darwinian narrative and ii) the construction of iconic failures as stand-alone phenomena in history. In terms of the construction of iconic failures, the cases of Paul Kammerer and Tofim Lysenko are described in detail. In this section I also consider the ‘closure rhetoric’ that has shaped late twentieth century perceptions of Lamarckian theory. I describe the historical representation of the ‘three fold closure’ of Lamarckian opposition, in which, ‘iconic closure’ of Lamarckian debate is attributed firstly to August Weismann, secondly to Luria and Delbruck and thirdly to Francis Crick and his central dogma of molecular genetics.

### 3.2.1: The Construction of Iconic Failures: Kammerer and Lysenko

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<sup>502</sup> Ghiselin, M. (1994)

### Paul Kammerer:

The standard narrative describing the life and work of Austrian biologist Paul Kammerer provides an example of the construction of fraud or 'iconic failure' in the history of science. Kammerer's story is of course not the only example of a narrative of scientific fraud and discredit<sup>503</sup>, however, certain of its features make it particularly memorable and lead to its retelling. Firstly, the nature of the alleged fraud lends the story to development as an iconic episode. Many stories of scientific fraud involve the forgery of complex calculations or the fraudulent interpretation of data. These cases are often difficult to describe and may only be meaningful to specialists. However, in Kammerer's case accusations focus on the doctoring of biological specimens. It is claimed that Kammerer tampered with specimens to give them an appearance that supported a Lamarckian interpretation of their evolution. Thus, the alleged fraudulence was uncomplicated, and requires no specialist knowledge to appreciate. Secondly, Kammerer's suicide subsequent to the accusations of fraud, and the fact that the case appears to a degree unsolved, lends the appeal of mystery, increasing the story's value as a popular tale. Thirdly, the story invokes the popular long-term conflict between Darwinians and Lamarckians, and seems to confirm the cultural identities of Lamarckism and Darwinism that appear on the old map.

The construction and deployment of this 'failure' narrative for Kammerer's Lamarckian work is interesting to this analysis on two levels. Firstly, the story acts in a service role to the triumphalist Darwinian narrative that was being propagated by agents of the synthesis. The accusations against Kammerer came in the 1920s, and provided a timely example for the synthesis authors of the disreputable nature of recent Lamarckian research. Indeed, the synthesis authors made some of the early accusations against Kammerer and begun the framing of this tale of scientific discredit (see below). Secondly, the story acts as part of a separate, but related, narrative describing the decline of Lamarckism in the twentieth century. This material does not relate directly to the triumphalist narrative of Darwinism, but rather acts alone as part of the broader historical discourse describing the fall of

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<sup>503</sup> For example, sociologists Gieryn and Figert have provided a detailed account of the fraud accusations made against psychologist Sir Cyril Burt in the 1970s and the construction of him as a failed or pathological scientist, see Gieryn, T. & Figert, A. (1986)

Lamarckism in the twentieth century. Thus, Kammerer's case is used to both assert the merits of Darwinism and also to confirm the demise of Lamarckism. In that regard, Kammerer's story contributes to the old map identities of both Lamarckism and Darwinism.

#### Paul Kammerer's 'iconic failure'

Between 1902 and 1926 Paul Kammerer investigated the inheritance of acquired characteristics at the prestigious Viennese Institute for Experimental Biology. He demonstrated remarkable experimental skill in the culture and breeding of reptiles and amphibians.<sup>504</sup> Kammerer was able to get these animals to breed in vastly modified controlled habitats, and was able to study the inheritance, over several generations, of the physical features that arose as a result. That is to say, he was able to test the heritability of features that had arisen during an organism's lifetime as a direct response to the requirements/pressures of their environment.<sup>505</sup>

Through experiments using salamanders, and perhaps most famously the Midwife Toad (*Alytes obstetricans*), Kammerer demonstrated the action of this Lamarckian process.<sup>506</sup> The best-known experiment, and the most significant to the narrative of 'failure', examined the inheritance of acquired secondary sexual characters in the Midwife Toad. Under natural conditions this species breeds in terrestrial habitats. During mating the male clasps the female above the back legs and collects and fertilizes the string of eggs that she releases. This act of coupling is standard for frogs and toads. However, the majority of species carry out this process in aquatic environments. To facilitate the coupling males develop rough pigmented patches on their front limbs during the mating season, enabling them to maintain a hold on the female throughout the process of external fertilization. These patches are called nuptial pads, and are absent in the midwife toad on account of its terrestrial lifestyle.

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<sup>504</sup> Koestler, A. (1971)

<sup>505</sup> Interestingly, Kammerer had not set out to demonstrate Lamarckian evolution. When he began his career he was a firm believer in the principles of Mendelism and Weismannism. Only as he bred new generations from his original stocks did he begin to perceive the heritability of the phenotypic modifications that had been induced in his specimens (Koestler, 1971).

<sup>506</sup> See: Kammerer, P. (1923) Breeding experiments on the inheritance of acquired characters. *Nature*, 111: 637-640; Kammerer, P. (1924) *The inheritance of acquired characteristics*. Boni & Liveright, New York.

Kammerer's experiments were designed to test the phenotypic effect of forcing these terrestrial toads to reproduce in an aquatic environment.<sup>507</sup> Kammerer claimed that after several generations in the aquatic environment male toads began to develop the nuptial pads characteristic of aquatic species. The development of these pads did not appear to occur by the process of random mutation and subsequent natural selection that Darwinian theory demands. Rather, the nuptial pads arose in direct response to the new environmental challenge. The report was made more contentious by Kammerer's claim that the change was heritable. He claimed that his results provided the first experimental evidence in favour of the inheritance of acquired characteristics.

The standard story of Kammerer's 'failure' centres upon a preserved midwife toad specimen from one of the later generations of his protracted experiment. This particular specimen was considered central to the credibility of his work, and from 1910 onwards sceptics began to request loans of this midwife toad with nuptial pads.<sup>508</sup> English Darwinian William Bateson was Kammerer's greatest critic.<sup>509</sup> He engaged in debate with Kammerer over a period of fourteen years, and maintained that verification of the authenticity of the midwife toad specimen was at the heart of the credibility of Kammerer's Lamarckian theories.<sup>510</sup> Not only did Bateson himself make repeated requests to examine the midwife toad, but he also encouraged others to try and get a close look at the material if they visited Vienna. We might consider these activities as an evidence of Bateson carrying out boundary work for the exclusion of Kammerer. From that perspective, Bateson's construction of a fraud case against Kammerer is a powerful tool for the achievement of his boundary work objectives. Kammerer biographer Arthur Koestler suggests that Bateson's tone when

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<sup>507</sup> Koestler, A. (1971)

<sup>508</sup> Koestler, A. (1971)

<sup>509</sup> See for Bateson's critique of Kammerer: Bateson, W. (1923a) Dr Kammerer's testimony to the inheritance of acquired characters. *Nature*, 112: 344-345; Bateson, W. (1923b) The inheritance of acquired characters in *Alytes*. *Nature*, 112: 391; Bateson, W. (1923c) Experiments on *Alytes* and *Cona*. *Nature*, 112: 899.

<sup>510</sup> That Bateson placed so much emphasis on this specimen troubled Kammerer. In his view the midwife toad experiments were not the most definitive Lamarckian evidences he had produced. Kammerer attributed the appearance of the nuptial pads to atavism, which was subtly different from the inheritance of acquired characters. Kammerer was always keen to encourage more attention towards his experiments on sea squirts, which he considered far more definitive [Koestler, A. (1971)]. Kammerer's Lamarckian work was eventually discredited on the basis of a reexamination of a specimen that he had never asserted was a definitive example of the inheritance of acquired characteristics. This adds a lamentable element of misunderstanding to Kammerer's tale.

giving these recommendations always implied that the specimen was likely not to be authentic.<sup>511</sup>

In 1923, Kammerer visited England and brought with him the last preserved midwife toad specimen to have survived the disruption of the war. Not only were specimens lost during the war, but also a lack of resources caused Kammerer's surviving lineage of toads to die off. The experiment had come to an end, and it would take many years and rare breeding skills to recreate the results. A key concern for Bateson, and also for Kammerer, was that the experiments had not been successfully repeated by any other researcher. However, this was because no other researcher had managed to breed the midwife toad in an aquatic environment.<sup>512</sup> During his 1923 visit Kammerer's last specimen was examined by researchers at Cambridge University and at the Linnean Society in London. The phenotypic appearance of the nuptial pads was verified and, they were also examined microscopically. Interestingly, Bateson did not attend the meeting at Cambridge and never examined the specimen during Kammerer's trip to England, yet he maintained his focus on the verification of the authenticity of this specimen after it was returned to Vienna.<sup>513</sup> Science journalist, Richard Milton, suggests that, at this time, 'a pronounced anti-Kammerer faction developed'. Led by Bateson, they mounted 'frequent attacks' on his work and implied that the 'experiments must be erroneous or fraudulent because the inheritance of acquired characteristics is impossible'.<sup>514</sup> Kammerer had only limited support, largely led by Ernest MacBride, one of the last supporters of recapitulation theory, and a staunch advocate of Lamarckism.<sup>515</sup>

In early 1926, G. K. Noble, curator of reptiles at the American Museum of Natural History, visited the Vienna Institute and requested to examine the midwife toad specimen. Koestler demonstrates that Noble had long been an ally in Bateson's campaign against Kammerer, and that Bateson encouraged the visit.<sup>516</sup> A few months after his visit to Vienna Noble reported his examination of Kammerer's specimen in

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<sup>511</sup> Koestler, A. (1971)

<sup>512</sup> Koestler, A. (1971)

<sup>513</sup> Koestler, A. (1971)

<sup>514</sup> Milton, R. (1994) *Forbidden science: suppressed research that could change our lives*. Fourth Estate, London.

<sup>515</sup> See: MacBride, E. (1924) *An introduction to the study of heredity*. Williams & Norgate, London.

<sup>516</sup> Koestler, A. (1971)

*Nature*.<sup>517</sup> He stated that a simple low magnification examination of the toad had revealed that the nuptial pads had been generated not by the inheritance of acquired characteristics, but rather by the injection of Indian ink into the epidermal tissue.

Shortly before these accusations were made Kammerer had been invited to transfer to Russia and build an institute for experimental biology.<sup>518</sup> In the weeks before Noble's accusation Kammerer had made preparations to move, arranging for his equipment to be packed and transported.<sup>519</sup> However, days before his transfer Kammerer committed suicide, apparently shooting himself on an Austrian mountain path. The note he left stated that the pressure of the accusations had been too much for him. Kammerer's suicide of course appeared as an admission of guilt.<sup>520</sup>

Almost immediately after Kammerer's death the story of his life and work began to be translated into an iconic tale of fraud and discredit. The effect of this translation was such that Arthur Koestler, writing in 1971, stated:

'Nobody who reads about Kammerer in current books on biology could believe in his innocence.' (Koestler, 1971:122)

Kammerer's 'iconic failure' was addressed by a variety of authors, interestingly, including some of the agents of the modern synthesis. In his 1926 text, *Essays in Popular Science*<sup>521</sup>, Julian Huxley dedicates a chapter to the consideration of the theory of acquired characters.<sup>522</sup> In this treatment he asserts that Lamarckian theories of the inheritance had become obsolete. He suggests that there might be means by which the 'germplasm' can be influenced by the environment, but that this would be a rare phenomenon. On the issue of Kammerer and his 'proofs' of Lamarckian

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<sup>517</sup> Noble, G. (1926) Kammerer's *Alytes*. *Nature*, 118: 209-211.

<sup>518</sup> This is especially important in light of the state of Russian biology in this period. Lamarckism was gaining popularity at this time, and just a few years later Tofim Lysenko would facilitate the rise of Lamarckism in Russia to state of institutionalization. Had Kammerer survived to take up this position he would have found a more permissive context in which to carry out his non-Darwinian work.

<sup>519</sup> Koestler, A. (1971)

<sup>520</sup> Burkhardt, R. (1998)

<sup>521</sup> Huxley, J. (1926) *Essays in popular science*. Chatto & Windus, London.

<sup>522</sup> In 1926 this was still a valid topic for an evolutionary text, with many biologists using some version of acquired character theory in their research. Only later, once the modern synthesis had been established, did it become less desirable to address Lamarckian theory. In his formative text, *Genetics and the Origin of Species* (1937), Dobzhansky recommended that Lamarckism be removed from materials authored as part of the synthetic project.

inheritance Huxley states:

‘ Now Dr. Kammerer claims himself to have experimentally demonstrated the inheritance of acquired characters in salamanders, in toads and ascidians. In the brief space I have at my disposal, and at the risk of seeming curt, I can only say that his work has not carried conviction to biologists as a whole, and in particular to those who ought to be best qualified to judge – the students of heredity, with Bateson, Baur, Morgan, Goldschmidt and Johannsen at their head. No-one has ever been able to repeat them, and distinguished workers like Herbst have obtained quite opposite results.’ <sup>523</sup>

In his 1942 text *Evolution: The Modern Synthesis*, Huxley makes plainer still his view of the discredit attached to Kammerer’s research. He explains:

‘Nor need I go in detail through the wearisome discussion of the various scientific ‘proofs’ of Lamarckian inheritance that have been advanced. I would merely say that subsequent work has either disproved or failed to confirm the great majority of them. An unfortunate suspicion rests on Kammerer’s work, and his results on salamanders have not been confirmed by Herbert (1924).’ <sup>524</sup>

These two examples show how allusions to Kammerer’s ‘fraud’ begin to be reframed as statements asserting the fact of that fraud. We might consider that Huxley co-opted the theme of Kammerer’s fraud to bolster the boundary work for Darwinian monopolization with which he had engaged.

Dobzhansky adds to the contemporary construction of failed Lamarckism, stating that:

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<sup>523</sup> Huxley, J. (1926) p.33-34. The extent to which this appraisal of Kammerer is a purposed construction rather than an account is made clearer if we consider Arthur Koestler’s (1971) reply to these accusations. Koestler demonstrates from archival sources and interviews that: Firstly, Kammerer thought that only his sea squirt experiments showed the inheritance of acquired characters. He did not claim the same of his salamander or toad research. Secondly, as I have mentioned above, Kammerer’s experiments had not been repeated because no other researcher had been able to raise amphibians in the manner required. In Huxley’s statement, quoted here, the implication is quite different.

<sup>524</sup> Huxley, J. (1942) p.458

‘Inheritance of acquired characters...apparently does not take place. The few remaining believers in this contingency, epigones of Lamarckism, can adduce no critical evidence in support of their convictions; at any rate up to the present their speculations have proved barren as working hypotheses.’<sup>525</sup>

Dobzhansky does not mention Kammerer or his work directly. By this device Dobzhansky renders Kammerer just another of those ‘epigones of Lamarckism’. This approach fits with the statement that Dobzhansky makes in the preface to this same text that: ‘Considerations of space have forced us to refrain from a detailed discussion of some of the objections that have been advanced against the genetic treatment of evolutionary problems. Thus Lamarckian doctrines find but a brief mention.’<sup>526</sup>

In 1946 biologist Richard Goldschmidt discussed the case, and added accusations that notebooks from Kammerer’s lab had been doctored. He stated: ‘...statements were found in Kammerer’s papers that did not tally. There had not been sufficient time, according to his own records, for the generations he claimed to have been bred.’ He explained that Kammerer had probably become ‘..so absorbed with the necessity for proving his claims that he started inventing results or ‘doctoring’ them.’<sup>527</sup> Kammerer’s iconic failure began to be deployed through educational textbooks and popular culture. Biologist Aronson Lester has pointed out that from as early as 1938 Kammerer’s story was ‘regularly told to biology students as an object lesson.’<sup>528</sup>

By the 1980s, Kammerer’s case was in use as a historical example of the ‘problems’ of Lamarckian support; his failure had become a landmark on the old map of Lamarckism. In *Evolution and the Diversity of Life*, Mayr says that hindsight allows us to interpret Kammerer’s positive results as fraud and, in the same volume of collected essays, Richard Burkhardt adds that Kammerer’s suicide appears as an admission of guilt.<sup>529,530</sup> Evolutionary biologist Steven Jay Gould retold the story of

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<sup>525</sup> Dobzhansky, T. (1937) p.18

<sup>526</sup> Dobzhansky, T. (1937) p.xi

<sup>527</sup> Goldschmidt, R. (1949) Research and politics. *Science*, 109: 219-227. p.221

<sup>528</sup> Lester, A. (1975) The case of the midwife toad. *Behaviour Genetics*, 5(2): 115-125

<sup>529</sup> Mayr, E. (1998) Preface. In: E. Mayr & W. Provine (Eds.) *The evolutionary synthesis*. Harvard University Press, Massachusetts.



the Indian ink in his 1980 popular essay collection, and added that: ‘...ironically Kammerer performed a Darwinian experiment without recognizing it...but concluded that he had demonstrated a Lamarckian effect’.<sup>531</sup> This construction adds stupidity to fraud.

Kammerer’s research seems to exemplify ‘pathological science’ of the kind that Irvine Langmuir<sup>532</sup> has outlined. That characterisation arises from the fact that his experiments appeared repeatable only by certain individuals, and only in certain ways. Since one of the hallmarks of science is replicability, Kammerer’s research offered an example of perversion of the scientific method and practice. It shows trainee scientists how science should not be. To support Kammerer’s findings in the 1920s demanded a degree of experimenters’ regress of the kind that Collins has described.<sup>533</sup> Experimenters’ regress allows that when an experimental repetition fails to produce positive results that failure is designated as a sign that the experiment has not worked, rather than as a sign that the underlying hypothesis is flawed. In Kammerer’s case, the regress centred around his special ability to raise experimental amphibian stocks. When others failed to replicate Kammerer’s observation he and his supporters attributed that fact to the inability of others to raise organisms in the altered environments.<sup>534</sup>

The treatment of Kammerer’s story by key agents of the modern synthesis and their supporters served to create a narrative of Lamarckian failure in a service role to the triumphalist Darwinian narrative. In their constructions Lamarckism is discarded and the inevitable outcome is the assertion of Darwinian authority. This construction of Kammerer also appeals to the popular enthusiasm for stories of fraud

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<sup>530</sup> Burkhardt, R. (1998)

<sup>531</sup> Gould, S. J. (1980) *The panda’s thumb*. Penguin, New York.

<sup>532</sup> Langmuir, I. (1989) Pathological science. *Physics Today*, 42: 36.

<sup>533</sup> Collins, H. (1974)

<sup>534</sup> It is worth noting that the designation of Kammerer’s approach as an example of experimenters’ regress does not take into account the role of ‘tacit knowledge’ in the achievement of certain experimental findings. Michael Polanyi [Polanyi, M. (1958) *Personal Knowledge*. Routledge and Kegan Paul, London] first described tacit knowledge, to describe how some experimental procedures rely on a significant degree of craft knowledge or skill that cannot be made explicit in written form and remains inarticulable. Jerrold Ravetz [Ravetz, J. (1971) *Scientific Knowledge and its social problems*. Oxford University Press, London] and Harry Collins [Collins, H. (1974); Collins, H. (1985)] have investigated further the way that tacit knowledge influences the production of knowledge, and the role that training has in transmitting craft skills. In the case of Kammerer his skills in the breeding of amphibians might represent such craft knowledge, and as such the designation of his work as pathological science may exist as a construction of the Kammerer fable, rather than as a valid analysis of Kammerer’s practice.

and discredit in science. The repetition of Kammerer's story in histories and textbooks is often a function of that enthusiasm.<sup>535</sup>

#### Tofim Lysenko: politicised iconic failure.

During the 1930s Russian scientist Tofim Lysenko became a powerful force in Russian science and politics. He promised a theory of genetics consonant with Russian political ideology. His evolutionary theories provided a biological basis for the hope that mass social change could be effected in only a few generations, with improvements being passed to future generations not only through education, but also through heredity. His primary concepts were taken directly from Paul Kammerer's troubled research. By invoking Lamarckian principles, Lysenko was able to confirm that the evolution of the Russian state would be progressive, and that the socialist state was within reach. Lysenko denounced survival of the fittest and Mendelian genetics as tools of the Bourgeois, used to stifle optimism for progress in human societies.<sup>536</sup> In place of these principles, Lysenko invoked the accumulation of acquired characteristics and altruism as the agents of evolution.

Although Lysenko enjoyed more influence than Kammerer, his work eventually became the focus of similar accusations and retrospective re-evaluative scrutiny. During the mid century it emerged that Lysenko had used political connections to bypass the requirement for stringent biological evidence. He was eventually denounced as an authority in 1965, after serious crop failures resulted from the application of his methods to agriculture.<sup>537</sup>

Robert Young has examined the representations of Lysenko and Lysenkoism that emerged during the twentieth century<sup>538</sup>, essentially examining the construction of Lysenko that became one of the major landmarks on the old map. Young argues that a standard narrative of Lysenko's failure has arisen, and that it has been perpetuated for two reasons. Firstly, the story of Lysenko's downfall illustrates a

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<sup>535</sup> For example, Arthur Koestler's treatment of the case in a self-contained narrative that is not related to the Darwinian agenda. [Koestler, A. 1971]

<sup>536</sup> Bowler, P. (1992)

<sup>537</sup> Milner, R. (1990) *The encyclopedia of evolution: humanity's search for its origins*. Facts on file, New York; Ravetz, J. (1990)

<sup>538</sup> Young, R. (1978) Getting started on Lysenkoism. *Radical Science Journal*, 6/7: 81-105.

popular theme in science studies; it acts as a cautionary tale concerning the ‘intrusion of the alien values of politics and ideology into the domain of value-neutral science’.<sup>539</sup> Secondly, representations of Lysenko tend to focus on the relationship of Lysenkoism to Stalinism, emphasising the use and abuse of power under that regime. This approach results from Western authors use of the Lysenko story as part of a broader standard narrative that defames communism. Young refers to this standard characterisation as ‘red-baiting’. He concludes that the combination of these authorial motivations has led to the production of treatments of Lysenkoism that are generally marked by ‘self-congratulation’ and use Lysenko as a cautionary tale against both the general abuse of science for political ends and communist politics specifically.<sup>540</sup>

Lysenkoism became an object lesson against the combination of science and politics.<sup>541</sup> Lysenko’s failure has been recalled and reinforced by subsequent authors. For example, Gould characterises the Lysenko affair as ‘appalling’.<sup>542</sup> The combination of Lamarckism and its enthusiast Lysenko is perceived to have set back Russian agriculture, and indeed society, thirty years; a harsh condemnation for an already defamed theory of evolution. Bowler suggests that this representation of Lysenkoism promoted the idea that ‘Lamarckians can behave as badly as anyone else.’<sup>543</sup> On the old map record of Lamarckism, the landmark of Lysenkoism records the stigma suffered by association with this historic episode.

By the end of World War II the state of biology, and in particular evolutionary science, had changed dramatically. Eugenics had been stigmatised irreversibly by the Nazi ethnic cleansing policies, and science itself had suffered from association with the politics and events of the war. The use of the A-bomb in

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<sup>539</sup> Young, R. (1978) p.81

<sup>540</sup> Young cites several classic treatments of Lysenko as examples of the perpetuation of this standard narrative: Joravsky, D. (1970) *The Lysenko affair*. Harvard University Press, Massachusetts; Medvedev, Z. (1969) *The rise and fall of T. D. Lysenko*. Columbia University Press, New York; Graham, L. (1973) *Science and philosophy in the Soviet Union*. Alfred Knopf, New York; Lecourt, D. (1977) *Proletarian science? The case of Lysenko*. NLB Press, London. He argues that Lewontin and Levins treatment of Lysenko is almost unique in escaping these problems, since, it retells the Lysenko story from the perspective of the Marxist history of science and so adopts a necessarily different perspective from that in the standard narrative. See: Lewontin, R. & Levins, R. (1976) The problem of Lysenkoism. In: H. Rose & S. Rose (Eds.) *The radicalization of science*. Macmillan, London.

<sup>541</sup> Bowler, P. (1992)

<sup>542</sup> Gould, S. (1980) p.67

<sup>543</sup> Bowler, P. (1992) p.458

Hiroshima and Nagasaki had transformed science in the public mind from being a noble endeavour into a servant of the state - a potentially uncontrolled threat to human life on a large scale. The technologies used to kill in wartime had tainted science with malign intent. The change of scale and funding had reinvented science as a slave to money, compelled to meet political objectives. Science since the war was irreversibly linked to national security.

It was no longer desirable to populations that social ideologies be justified through association with scientific theory. The manner in which Lamarckism had been utilised in Russia in the twentieth century was no longer inviting. In this climate, Lamarckism suffered considerable decline; without the benefits of its progressive view as a tool for social policy planning, Lamarckism had little to offer. The links to social policy that some Neo-Lamarckians had forged now appeared to be fraught with moral and ethical problems that could lead to the misuse of science. Neither Kammerer nor Lysenko had provided any sound evidence that Lamarckism operated at a molecular, or indeed organism level. Thus, there was no impetus to seek Lamarckian phenomena either in the lab or in the field. Not only had Kammerer and Lysenko lost their contests for authority, but also the culture of distrust for links between science and ideology meant that further authority was withdrawn from them. Not only did those 'inside' science reject their work, but also the inhabitants of adjacent cultural territories agreed that such work should not be included as 'science'.

The Kammerer and Lysenko episodes appear to confirm that adherence to Lamarckian principles is unwise. It seems that Kammerer was encouraged towards fraud because he could not generate genuine physical evidence of Lamarckism. Also, it appears that Lysenko followed bad practice and used political alliances to protect his position because he also was not able to generate results. Irrespective of the historical accuracy underlying these constructions, they are nevertheless recorded on the old map. The inclusion of these constructions as landmarks forms part of the record of stigmatisation and defamation. That record remains for reference during new contests.

On the old map, the stories of Kammerer and Lysenko further assert the success of Darwinism. While Lamarckians have exhibited weakness in experiment,

the Darwinians have been rigorous, for example, in population studies and *Drosophila* genetics. The identity of Lamarckism on the old map was contributed to by William Bateson's boundary work and fraud accusations against Kammerer, the synthesis authors reinforcement of those accusations, and finally by the cultural stigma of Lysenko's marriage of science and political ideology. Where the Kammerer narrative had added fraud as a landmark, the Lysenko story added abuse of power and political corruption. The old map features Kammerer and Lysenko not only as failures in their own contests for authority, but also as exemplars of the stigma attached to Lamarckian sympathy; the construction of these two individuals as frauds stigmatised the act of Lamarckian support.

### 3.2.2: Some other features of the old map identity of Lamarckism.

Michael Ghiselin has examined the fate of Lamarck and Lamarckism during the twentieth century.<sup>544</sup> Although Ghiselin does not use Gieryn's 'old maps' language, he is nevertheless effectively examining the construction, form and deployment of the old map identity of Lamarckism. Ghiselin uses school textbooks to demonstrate the construction and perpetuation of what he calls 'the imaginary Lamarck'. He argues, as I do here, that the construction of Lamarckism has been undertaken in part in a service role to the triumphalist construction of Darwinism. He suggests that the Lamarck in school textbooks is used as a straw man for Darwinism to topple. He adds that this tactic demands that a fictitious Lamarckism be invented i.e. one that enables comparison and contrast with Darwinism. This leads to what Ghiselin calls the development of a 'false dichotomy' between Lamarckism and Darwinism. Cultural perception of that constructed dichotomy is perhaps one of factors that makes the old map identities of Lamarckism and Darwinism so persistent between conflict episodes, and thus gives such endurance to the authority allocations that the map records. I argue that the Lamarckism constructed to serve this dichotomy has contributed several key features to the old map.

The Darwin versus Lamarck dichotomy that Ghiselin traces in school textbooks relies upon the construction of Lamarck as a precursor to Darwin, with

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<sup>544</sup> Ghiselin, M. (1994)

Lamarckism superseded by Darwinism. This adds a record of 'abandonment' to the record of discredit I have described above. Ghiselin explains that, to act as a precursor, Lamarck is constructed as the architect of a theory of evolution, the originator of the theory of acquired characteristics and as a natural scientist. However, he points out, Lamarck neither originated the notion of evolution nor that of the inheritance of acquired characters. He presented his views on evolution alongside little scientific evidence, and his work was regarded at the time as philosophy rather than science. Ghiselin argues that Lamarck was principally a metaphysician, and that his interest in evolution was born out of that enthusiasm. He states that:

'Textbooks pit Lamarck against Darwin in a mythical contest from which Darwin emerges victorious. To perpetuate that myth, the textbook-writers lead students to believe that Lamarck embraced the inheritance of acquired characteristics, that Darwin rejected it, and that this is the crucial difference between the two men's ideas about evolution.'

Irrespective of the historical accuracy of this presentation the old map nevertheless comes to record Lamarckism as an outmoded precursor of Darwinism. For Ghiselin this goes as far as to imbue Lamarckism with a significance it never had.

Ghiselin describes how the Darwin versus Lamarck dichotomy relies on certain common rhetorical devices, the perpetuation of which he traces through a succession of school textbooks. The most common of these is the trivialising of Lamarck's theoretical work, with the clearest example being the story of the giraffe's neck. That story suggests that Lamarck proposed that giraffes developed long necks after successive generations of the species stretching and elongating their necks as they reached for leaves in taller and taller trees. Through this process of use, and the acquiring of characters, each new generation arrived with a longer neck than the last. Ghiselin demonstrates that Lamarck's theory is often illustrated using only the example of the giraffe's neck. This is perhaps the most iconic feature of the construction of Lamarckism, and the story has been retold in countless introductions to evolutionary theory. However, as Ghiselin points out, Lamarck never proposed

the case of the giraffe as proof of the theory of acquired characters, rather he suggested it briefly as a hypothetical example of how evolution might occur. In fact, the English translation of Lamarck's 1809 *Philosophie Zoologique* is 405 pages long, and the giraffe reference appears only once and is two lines long.<sup>545</sup> Furthermore, Charles Darwin discusses the giraffe's neck in the sixth edition of *The Origin of Species*. Ghiselin says that, in spite of this, textbook authors have been recycling the iconic oversimplification of the giraffe story for decades to indicate the inadequacy of Lamarckian theory. Historian of science Ron Roizen suggests that the perpetuation of the giraffe story, and its use as a tool for the trivialisation of Lamarckian theory, has led to the fact that 'for most of us, Lamarckian evolutionary theory has been reduced to the bogus example of the giraffe's neck'.<sup>546</sup>

The dichotomy between Darwinism and Lamarckism is further enforced by isolating one theory from the other (except in the link manifest between precursor and successor), and pronouncing them mutually exclusive theories. Ghiselin argues that this impression of isolation is promoted by the fact that few textbook treatments refer to Darwin's own support of a theory of acquired characters. Darwin refers to Lamarck and was well aware of the content of his work. Therefore, rather than isolation, there is instead a degree of intellectual continuity in the work of these two theorists. In the construction of Lamarck as a precursor of Darwin it seems that the fifty years between the two men's contributions divided them completely. However, this does not take account of the continuum of evolutionary thought that existed in the intervening period. Lamarck and Darwin are far more intellectually and historically linked than the textbook representations generally imply.

So, the constructions of Kammerer and Lysenko's iconic failures appear as landmarks on the old map, denoting fraud and recording the legacy of discredit. Meanwhile, the map is added to by other kinds of authors who emphasise historical abandonment over discredit. These constructions serve a role in the construction of the triumphalist narrative of Darwinism, but they also contribute to a stand-alone identity of Lamarckism that was being created simultaneously. What Ghiselin

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<sup>545</sup> Lamarck, J. B. (1809)

<sup>546</sup> Roizen, R. (1971) *The argument of philosophie zoologique*. History of science seminar at Berkley, California. Transcript available at: [www.roizen.com/ron/lamarck.htm](http://www.roizen.com/ron/lamarck.htm)

considers to be the perpetuation of an identity of Lamarck by ‘successive teams of plagiarists’ is, in the terms of this chapter, perpetuation of the old map identity of Lamarckism. The replication of the construction of failed Lamarckism shows how the identity/authority of Lamarckism achieved the cultural stability and persistence that Gieryn sees as the hallmark of an old map.

### 3.2.3: The three deaths of Lamarckian theory and the rise of dogma in evolutionary biology: closure rhetoric perpetuates the old maps of Darwinism and Lamarckism

In this section I describe the three ‘deaths’ of Lamarckism that appear on the old map: August Weismann’s germ/soma divide, Luria and Delbruck’s fluctuation test and Francis Crick’s central dogma. These three major refutations are important landmarks on the record of the decline and discredit of Lamarckism, and each is represented as the final blow for the theory. Attempts to resurrect Lamarckism have been made subsequent to each refutation, and the old map identity of Lamarckism has been deployed to control those attempts.

The significance attached to these refutations resulted in the rise of certain Darwinian dogmas in evolutionary biology. I argue that these dogmas are recorded on the old map as confirmations of the monopoly of Darwinism in evolutionary biology. These dogmas assert the validity of the refutations and ensure their perpetuation as features of the old map.

#### August Weismann shuts down Lamarckism.

August Weismann is credited as having achieved the first experimental refutation of Lamarckism. In 1893, Weismann published the results of mouse experiments that apparently confirmed the failure of Lamarckian heredity and environment directed evolution.<sup>547</sup> A protracted study of the heritability of tail amputation had lead Weismann to conclude that environmental influences upon a parent generation could not effect the phenotype of offspring; traits acquired during a lifetime could not be passed through heredity to subsequent generations. Weismann identified a division between the reproductive cells and the body cells,

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<sup>547</sup> Weismann, A. (1893) *The germ plasm: a theory of heredity*. Scott publishing Company, London



which he defined as the germ/soma divide. He branded the germ cells immune to the effects of somatic cell modification by the environment.<sup>548</sup> That barred the route for the inheritance of acquired characters.

In the turn-of-the-century context these results enjoyed enhanced credibility because they had been generated through experimental work. At that time practical experimentation was regarded as the best route for understanding heritability and adaptation. In the relation to the old map record, Weismann's work appears to have precipitated decline in Lamarckian support around the turn of the century. As Bowler puts it: 'Weismann's theory had a crucial implication for evolution: it made Lamarckism impossible'.<sup>549</sup> However, I have mentioned above that no such decline in Lamarckian support was evident at that time. Bowler acknowledges the actual contemporary impact of Weismann's work: 'Far from putting Darwinism on a firmer footing, his campaign backfired and ignited a wave of anti-Darwinian feeling that swept through biology in the decades around 1900.' The effect of this was that: '...many biologists preferred to move into the Neo-Lamarckian camp.'<sup>550</sup> So Weismann's work, which is credited historically as a major blow to Lamarckism, in fact had the opposite result. His refutation has been constructed subsequently to support the image of a steady rise of Darwinism and steady decline in Lamarckian support.

What can a broader view of Weismann's work tell us? Weismann had studied a mutilation effect, and concluded from this the invalidity of the 'use/disuse' theory. While his concept of the germ/soma divide may have been legitimate, the existence of that divide could not be tested through the inheritance of mutilation. Weismann failed to test the results of use/disuse effects, and instead tested for the inheritance of injury. Despite these methodological problems, Weismann's work has nevertheless been constructed as precedent against Lamarckian inheritance. When Edward Steele published his theory of acquired immunity in 1979<sup>551</sup>, Weismann's barrier was invoked as one serious impediment to the acceptance of the results.<sup>552</sup> Similarly, in

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<sup>548</sup> Maynard-Smith, J. (1993b)(3rd edition)

<sup>549</sup> Bowler, P. (1989)(revised edition) p.251

<sup>550</sup> Bowler, P. (1989) p.117

<sup>551</sup> Steele, E. (1979).

<sup>552</sup> Pollard, J. (1984) Is Weismann's barrier absolute? In: Mae-Wan Ho & P. Saunders (Eds.) *Beyond Neo-Darwinism: an introduction to the new evolutionary paradigm*. Academic Press, London.

the case of directed mutation, Weismann was again cited as an example of precedent against the claims.<sup>553</sup> It seems, therefore, that in spite of the practical problems of Weismann's experiment, its results were incorporated into the persistent identity of the legitimacy of Lamarckism. The old map summarizes all the contests that Lamarckism is perceived to have lost, and Weismann's results were constructed as a key one of those losses.

Michael Ghiselin suggests that Weismann's experiments have been constructed as an iconic refutation of Lamarckism.<sup>554</sup> From the time of their publication they have been cited as a key disproof of Lamarckism, and that recurrent citation has gained its own momentum and persistence through replication. Ghiselin states that Weismann's experiments have been 'mindlessly cited in many textbooks' to exemplify the 'tests' that Lamarckism has failed. Ghiselin suggests that textbook authors are effectively required to demonstrate how the contest between Lamarckism and Darwinism was decided in favour of Darwinism. He argues that they use the notion of 'tests' to give iconic examples of Lamarckian failure. The three examples of closure that I discuss in this section have been constructed and perpetuated on account of their usefulness as examples of 'tests' affirming the superiority of Darwinism.

#### Salvador Luria and Max Delbruck shut down Lamarckism (again).

In 1943, Luria and Delbruck published results of bacterial experiments that appeared to rule out mutation as a directed or adaptive response to environmental change.<sup>555</sup> They had raised bacteria in media containing bacteriophage virus; an agent of lethal infection. The only bacteria able to survive the infection would be those that had achieved resistance through mutation. The aim of the assay was to test whether these resistant mutants arose as a result of directed adaptation influenced by the infection, or at random times during the culture irrespective of the presence of the infectious agent. To support the Darwinian model these mutations would have to occur at random, as a chance mutation that happened to confer benefit where it arose in the company of bacteriophage. Alternatively, in support of directed mutation,

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<sup>553</sup> Lenski, R., Slatkin, M. & Ayala, F. (1989)

<sup>554</sup> Ghiselin, M. (1994)

<sup>555</sup> Luria, S. & Delbruck, M. (1943)

increased levels of resistance mutation would occur subsequent to the introduction of infection.

After culturing the bacteria alongside the bacteriophage, the researchers measured the quantity of resistant mutants in each colony. They created the ‘fluctuation test’ as a statistical analysis of the distribution of the resistant mutants.<sup>556</sup> This test predicted that the distribution resulting from random (not environment directed) mutation would appear as a ‘normal’ distribution, or as a bell curve. With random mutation the number of mutants would vary widely between individual replica-plated cultures, depending upon the point during the culture time at which the chance resistance mutation arose. A colony experiencing the beneficial mutation early on would contain a large number of resistant mutants when replica plated, since these would have had a chance to reproduce. Conversely, in a culture where the beneficial chance mutation occurred late on or not at all there would be far less resistant individuals in the replica-plated culture. The ‘normal’ distribution would therefore indicate that the resistance mutation arose at random during bacterial culture, and not at a common point determined by the introduction of the bacteriophage as a selective agent.

Luria and Delbruck’s 1943 paper reported that the bacteriophage resistance mutation exhibited this normal distribution; that is, bacterial mutation is random with respect to selective pressure. When the Harvard directed mutation research was published decades later, Luria and Delbruck’s fluctuation test was invoked as precedent against the results. That invocation appeals to the authority attached to Luria and Delbruck on the old map, and suggests that the new conflict be resolved on the basis of existing authority allocations. Cairns’ team had anticipated this, and dedicated space in their first paper to a lengthy description of the difference between their methodology and that used by Luria and Delbruck. They argued that the 1940s work had been undertaken using a lethal agent as the selective force. This meant that the bacteria had no chance to evolve to suit the conditions since they were killed at once unless they were resistant.

Luria and Delbruck’s research embodied a problem similar to Weismann’s. The effect concluded upon was not the one that had been tested. Luria and

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<sup>556</sup> Luria, S. & Delbruck, M. (1943)

Delbruck's assay did test for the frequency of phage resistance as a random mutation, however, it did not allow for a phase of adaptive mutation and yet concluded that it did not exist. Cairns' experiments differed in that they impeded the growth of the bacteria rather than killing them. This left the bacteria alive in a stationary, non-reproductive state and with an impetus to undergo change, adapt, and re-enter growth phase.

Luria and Delbruck's results are presented historically in much the same way as Weismann's. Their fluctuation test is acknowledged as final definitive evidence against adaptive mutation, and thus Lamarckian environment directed evolution. Interestingly, alongside the construction of Weismann's work, the 'fluctuation test' represents the second lethal blow to Lamarckian theory - which would appear as a rather redundant achievement. The reason for this apparent inconsistency in the historical presentation of this double termination of Lamarckism is that only half of the story of this 'closure' is being told. The presentation of Weismann's work as conclusive is deceptive. In fact, as described above, his test was considered by many to have been invalid from its very premise. The traditional narrative describes Weismann's publication as the point of the demise of Lamarckism, and yet history betrays the presence of an active community of Neo-Lamarckians who attributed very little importance to his research. The construction of a second demise of Lamarckism is necessary because the theory had not been closed down by Weismann's work. By the 1940s, with Lamarckism at full strength in Russia, it had again become necessary to attempt the kind of definitive refutation of Lamarckian principles that the old map records Weismann had already achieved.

#### The 'Central Dogma' Shuts down Lamarckism (for the third time)

The rise of molecular genetics led to the proposal of extensions to the rules of heredity. In particular, the principle of unidirectional information transfer from genetic material to proteins was reinforced by developments from the 1940s onwards. In 1958, Francis Crick, co-discoverer of the structure of DNA, re-expressed this principle in the rhetoric of his theory of genetic structure:

‘The transfer of information from nucleic acid to nucleic acid, or from nucleic acid to protein may be possible, but transfer from protein to protein, or from protein to nucleic acid is impossible.’<sup>557</sup>

Francis Crick termed this principle the ‘Central Dogma of molecular genetics’.<sup>558</sup> He, and others, believed this principle was one of the constants of genetics, and that it barred communication between the environment and the genome. Crick later admitted that he had not been aware of the exact meaning of the word ‘dogma’ when he had coined this phrase, and that connotations of blind faith and unquestioning belief had been unintentional.<sup>559</sup> Despite this error the reception of his ‘dogma’ was in fact concurrent with its literal sense. It became self-fulfilling; a justification for the unidirectional transfer of information from DNA to protein, rather than a description of this apparent phenomenon. An example of the self-fulfilling nature of this dogma is provided in John Maynard-Smith’s *The Theory of Evolution*:

‘It follows that Weismann’s views must be accepted and Lamarck’s rejected, provided that two points can be established: i) that the central dogma of molecular biology is true, and ii) that the changes in the structure of organisms induced by changes in their environment cannot be transmitted direct to the next generation, without first being translated into nucleic acid.’<sup>560</sup>

This statement betrays the significance attached to Crick’s dogma, especially in terms of its role as a refutation of Lamarckian principles.

The ‘central dogma’ thus appears as the third ‘final blow’ for Lamarckism. The combination of Crick’s certainty regarding unidirectionality, with the constructions of Weismann and Luria and Delbruck, rendered debate on environment to DNA communication closed in the mid-late twentieth century.<sup>561</sup> The deployment

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<sup>557</sup> Crick, F. (1958a) The biological replication of macromolecules. *Symp. Soc. Exp. Biol.*, XII: 138

<sup>558</sup> Maynard-Smith, J. (1993b)(4<sup>th</sup> Edition)

<sup>559</sup> Judson, H. (1996) *The eighth day of creation*. Cold Spring Harbor Laboratory Press, New York.

<sup>560</sup> Maynard-Smith, J. (1993b) p.80

<sup>561</sup> This denial of environment to DNA communication endured in spite of Temin & Baltimore’s 1970 [Temin, H & Mitzutani, S. (1970); Baltimore, D (1970)] discovery of the enzyme reverse

and repetition of these ‘closure’ episodes rendered directed mutation inevitably controversial. To a community that had experienced these three refutations the very concept of working on a phenomenon that relied upon environment to DNA communication was contentious. These three closure episodes had come to feature on the old map, and as that old map was unfurled during the directed mutation debate it seemed to offer immediate refutation of that new challenge.

### **3.3 Conclusion: the cultural and intellectual context for non-Darwinian research in the late twentieth century.**

What does this tell us about the context for the directed mutation debate, and how does this help us understand the activity we see in that controversy? A key aim of this project is to identify forces of controversy perpetuation. To what extent can the social and cultural context of the directed mutation debate be attributed as an agent of controversy protraction?

By the late twentieth century the authority of Darwinism had achieved persistence between conflict episodes. A stable cultural identity of Darwinism had emerged based upon the numerous perceived successes of the theory and its proponents. An old map identity of Darwinism had emerged, constructed during the modern synthetic period, deployed and strengthened during the centennial celebrations and perpetuated through the activity of numerous authors in the later century. The old map summarised Darwinian successes during the numerous contests for authority in evolutionary biology. By the late twentieth century Darwinian theory was the only explanatory tool for evolutionary work in widespread use. Furthermore, Darwinism had not only achieved authority in relation to the intellectual community. The boundary work of the synthesis authors, and the work of subsequent textbook authors had created and deployed a popular cultural identity of Darwin and Darwinism. Even beyond the academic arena Darwinism had become synonymous with evolution. Charles Darwin had been constructed as the theory’s

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transcriptase, which transforms RNA into DNA – making possible half of the journey between protein and DNA that Crick’s Dogma rules out. The dogma has persisted, even though, as biochemist Laurence Moran has put it, ‘the demise of the central dogma of evolutionary biology is becoming an annual event.’ [Moran, L. (2007) Personal Blog at [www.sandwalk.blogspot.com](http://www.sandwalk.blogspot.com).]

founding father, and his endeavour had been framed to fulfil the criteria for the public understanding of genius in science. The old map of Darwinism had been constructed, deployed and perpetuated; it confirmed the authority of Darwinism and was ready to be unfurled at once in the directed mutation conflict as a point of reference for deciding the outcome of this new contest.

Meanwhile, Lamarckism had been stigmatised, and the old map recorded its decline. Associations with fraud had been made both as part of the Darwinian triumphalist narrative and also as part of the separate histories of Lamarckian decline and pathological science. Textbook authors and popular writers constructed Lamarckism as a precursor of Darwinism, and described its eclipse by the newer theory. The old map, therefore, also became a summary of the abandonment of Lamarckism in favour of Darwinism. Finally, episodes of iconic closure of Lamarckism became embedded in intellectual and cultural understandings of the history of Lamarckian opposition. The old map came to include the three refutations of Lamarckism (Weismann, Luria and Delbruck and the central dogma) as key features of the landscape of discredit.

The result of all this is historically interesting. When we view the history of evolutionary biology we do so through the lens of the old map images of Lamarckism and Darwinism. We see Darwinian triumph versus the decline of Lamarckism. However, historically twentieth century evolutionary biology in practice does not reflect those old map assertions. In spite of the accumulated closures of Weismann, Luria & Delbruck and Crick's dogma, theorists have continued to discuss Lamarckism and even attempt its resurrection. Since each of the closures was flawed in important ways, the ultimate refutation of Lamarckism seems incomplete.<sup>562</sup> Late twentieth century evolutionary biology was in practice more pluralistic than the old maps reveal. Bowler acknowledges this stating: 'the orthodox historiography of the Darwinian Revolution passes over the anti-Darwinian theories as irrelevant. They do not lie on the main line of conceptual development from Darwin to the modern genetical theory of natural selection...'.<sup>563</sup>

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<sup>562</sup> Weismann tested mutilation not acquired characters, Luria and Delbruck killed their bacterial subjects before they could evolve, and the discovery of reverse transcriptase exploded half of Crick's dogma.

<sup>563</sup> Bowler, P. (1989) p.92

Even as the synthesis was being constructed Lamarckians remained active. In the 1940s and 1950s Waddington remained a vocal Lamarckian and authored on acquired characteristics.<sup>564</sup> There has been continuity in twentieth century Lamarckian work, for example, with researchers recently providing details of the molecular basis for the theoretical claims that Waddington had made decades before.<sup>565</sup> Edward Steele's theory of acquired immunity has been accepted, and Cairn's Harvard team keenly read Lamarckism into their bacterial observations. And, as a foil to ultra-Darwinian advocates such as Mayr, Dawkins and Dennett, there have been vocal Lamarckian supporters. Edward Steele is of particular note.<sup>566</sup> There are many other examples of late twentieth century Lamarckian work besides these. In fact, enough that Burkhardt has announced them 'too numerous and diverse' to be dealt with effectively in his 'short paper' on Lamarckism in Britain and the United States.<sup>567</sup> Bowler attributes historians' neglect of these alternate theories to the 'imbalance created by the Darwin industry in the history of science'.<sup>568</sup> What this reveals is that in the case of old maps, the map and the terrain do not necessarily agree. That further illustrates the extent to which old maps themselves are constructed tools for serving agendas, rather than historical records. Old maps are created by boundary work, and then they become tools of boundary work.

It is a larger project than can be undertaken here, but there remains a history of late twentieth century Lamarckism to be recovered from the skewed history that the old maps promulgate. Bowler has attempted to recover some of that history, rehabilitating the 'forgotten anti-Darwinians'. He urges that 'a thorough study of

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<sup>564</sup> See for example, Waddington, C. (1942) Canalization of development and the inheritance of acquired characters. *Nature*, 150: 536-565; Waddington, C. (1953) Genetic assimilation of an acquired character. *Evolution*, 7: 118-126.

<sup>565</sup> Rutherford, S. & Lindquist, S. (1998) Hsp90 as a capacitor for morphological evolution. *Nature*, 396: 336-342; McLaren, A. (1999) Too late for the midwife toad. Stress, variability and Hsp90. *Trends in genetics*, 15(5): 169-171.

<sup>566</sup> However, the fate of these 'hidden' Lamarckians demonstrates the punishments exacted on those that defy the old map declarations. In Steele's case he has lost credibility, university affiliation, and is in a court battle to defend his professionalism. See: Steele, E. (1979) *Somatic selection and adaptive evolution: on the inheritance of acquired characteristics*. Williams & Wallace International, Toronto, Ontario; Steele, E., Lindley, R. & Blanden, R. (1998) *Lamarck's signature: how retrogenes are changing Darwin's natural selection paradigm*. Allen & Unwin, St Leonard's, Australia; Steele, E. (2000) The evidence for Lamarck. *Quadrant*, XLIV (364): 47-56.

<sup>567</sup> Burkhardt, R. (1980) p.343

<sup>568</sup> Bowler, P. (1989) p.93



these theories is essential if there is to be an accurate understanding of how evolutionism developed...'.<sup>569</sup> Historians have begun to show the way, developing a suspicion concerning the traditional (or old map) characterisation of Lamarck's theory and his construction as a defeated precursor for Darwin. Work has begun to dispel some of the myths that the old maps have introduced.<sup>570</sup>

On account of all this, in 1988, the suggestion of a non-Darwinian molecular process was profoundly problematic. It went not only against the consensus of the academic community, but also against the consensus recorded in the cultural cartography more widely regarding the authority attributed to Darwin and Darwinism. In addition, not only had Cairns and his team proposed a non-Darwinian model, but they had also framed their work as a Lamarckian resurrection. This alliance with Lamarckism invited the stigma of unorthodoxy. Reference to the old map confirmed for the opponents of directed mutation that Lamarckian resurrections had already been rejected in many contests. The proponents of directed mutation had attached themselves to a legacy that seemed to automatically invalidate their claims.<sup>571</sup>

The degree, stability and persistence of the cultural authority attached to Darwinism in the late twentieth century determined the severity of antagonism that greeted the directed mutation publications.<sup>572</sup> The old map recorded Darwinian

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<sup>569</sup> Bowler, P. (1989) p.93

<sup>570</sup> See for examples, Ghiselin, M. (1994); Barthelemy-Madaule, M. (1982) *Lamarck and the mythical precursor*. (trans. M. Shank) MIT Press, Cambridge, Massachusetts.

<sup>571</sup> Cairns, Overbaugh & Miller's (1988) alliance with Lamarckism appears counter intuitive. It seems unwise to have invoked the legacy of Lamarckian resurrection when we consider the stigma that this necessarily attached to their work. Their motivations for this alliance are explored in chapter 4.

<sup>572</sup> Other 'non-Darwinian' theoretical work met with similar antagonism in the late twentieth century. Of particular note: The theory of acquired immunity [Gorczynski, R. & Steele, E. (1981) Simultaneous yet independent inheritance of somatically acquired tolerance to two distinct H-2 antigenic halotype determinants in mice. *Nature*, 289: 678-681; Steele, E. & Gorczynski, R. (1981) Inheritance of acquired somatic modifications of the immune system. In: T. Hraha & M. Haskete (Eds.) *Cellular and molecular mechanisms of immunological tolerance*. Marcel Dekker, New York] Of which Walter Fitch stated: 'The reason for there being little concern among Darwinists for this rather common inheritance of acquired characters is that 'the inheritance of acquired characteristics is more of a slogan that captures part of the Lamarckian spirit than a statement of distinctiveness from Darwinism.' [Fitch, W. (1982) The challenges to Darwinism since the last centennial and the impact of molecular studies. *Evolution*, 36: 1133-1143. p.1137] Also, the paleontological theory of punctuated equilibria and macro-evolution, which states that the Darwinian theory of 'gradualism' in evolution is not supported by the fossil record, which instead betrays periods of rapid evolution interspersed with periods of evolutionary stasis. [Eldredge, N. & Gould, S. J. (1972) Punctuated equilibria: an alternative to phyletic gradualism. In: T. Schopf (Ed.) *Models in paleobiology*.

theory as orthodoxy. The same map also recorded the triumph of the theory over Lamarckism in a succession of contests. The directed mutation work was therefore necessarily controversial since it conflicted with the 'records'. The invocation of Lamarckism by the Harvard team meant that what might have been a debate in molecular biology, instead necessarily became an incarnation of the much longer running Darwinism versus Lamarckism conflict. What might have been a local and episodic contest became part of a continuum of dissent, and was linked to a historical legacy of conflict. Rather than the debate involving the drawing and redrawing of boundaries, instead old maps were unfurled and used as points of reference for this new contest. At once, decades of precedent became relevant to the negotiation of this anomaly, and the debate took on a scale and protraction much in excess of what would have been likely without the invocation of this legacy of evolutionary debate.

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Freeman, San Francisco.] Which Fitch disregards, saying: '...I can only conclude that punctuated equilibria are quite consistent with Darwinism.' [Fitch, W. (1982) p.1140]

#### **Chapter 4: The role of ‘the clash of sub-disciplines’ and ‘advocacy’ in the perpetuation of the directed mutation debate.**

In this chapter, I examine a further two of the six perpetuating forces that I identify in this project: ‘the clash of sub-disciplines’ and ‘advocacy’. In section 4.1 I describe how an inter-disciplinary contest, between molecular biologists and evolutionary biologists, contributed to the protraction of the directed mutation debate. I suggest that Gieryn’s boundary work theory can be applied to analyses of contests between groups of science specialists as effectively as it can be used to describe contests between science and other cultural domains. In section 4.1.2, I attempt to characterise molecular biology and evolutionary biology to determine their different styles, and attribute some controversy perpetuation to their different approaches. In section 4.1.3 I examine an analogous case of disciplinary conflict: the clash of chemists and physicists during the Cold Fusion debate.

In section 4.2 I deal explicitly with the role of John Cairns in the directed mutation debate. I examine his role as an advocate, and seek the boundary work motivations and interests that underpinned his approach. In section 4.2.1, I offer a biography of this key participant, and locate his involvement with this controversy within his long career as a prestigious and esteemed scientist. In section 4.2.2 I use the language Merton’s ‘norms’ to illustrate that Cairns retained his attention to his obligations as a scientist even though he chose to engage with unorthodoxy. In section 4.2.3 I examine the boundary work carried out by the advocates and adversaries in this conflict in more detail, and in particular, I attempt to explain why Cairns chose to make a Lamarckian association. In section 4.2.5, I contrast Cairns style of advocacy with that of Barry Hall, characterising them as ‘loud’ and ‘quiet’ advocates respectively. I argue that Cairn’s particular style (loud) increased the life-expectancy of the contest, perpetuating negotiation as a tactic of his advocacy. In section 4.2.6, I describe the analogous case of Stanley Pons and Martin Fleischmann’s ‘loud’ advocacy versus Steve Jones ‘quiet’ advocacy in the Cold Fusion debate.

#### **4.1 The clash of disciplines.**

The directed mutation anomaly arose in the context of molecular biological research, yet the implications of the anomaly were constructed as a problem for Neo-Darwinian evolutionary theory. Therefore, the debate precipitated a clash between the disciplinary groups 'molecular biology' and 'evolutionary biology'. The majority of participants in the debate are members of one or other of those communities, and the core-set is composed of advocates of directed mutation who are molecular biologists and their adversaries, who are evolutionary biologists.

##### **4.1.1 Molecular biology versus evolutionary biology.**

The delineations that separate science into sub-disciplines have been studied from a variety of perspectives. The oldest approaches are essentialist, and assume that science is divided into disciplines because nature itself is divided into aspects.<sup>573</sup> Sociological approaches have arisen more recently. The earliest of these were based on interest theories, and they considered that scientists operated in disciplinary groups as a strategy for competing effectively for scarce resources. In the interest models, groups of scientists, or individuals, compete for publication priority, funding and public esteem. The majority of interest models focussed on the competition between individuals, although they also often took into account the motivations for disciplinary associations. For example, when two or more disciplines are involved in a contest the practitioners of each branch of science act as a 'corporation' and compete in groups.<sup>574</sup>

With the rise of the sociology of scientific knowledge (SSK) disciplinarity began to be looked at in new ways, particularly from a constructivist perspective. Some of the SSK studies looked at the demarcation of science from non-science, and it was from that attention that boundary work theory and cultural cartography arose. However, boundary theory has rarely been extended to address the processes by

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<sup>573</sup> McAllister, J. (1992)

<sup>574</sup> McAllister, J. (1992)

which the internal delineations of science are determined and maintained. As Thomas Gieryn has noted, the studies have focussed on the delineation of science from non-science and the activities that determine that demarcation.<sup>575</sup> Recently, contributions from cognitive sociology have turned further attention on the disciplinary demarcations.<sup>576</sup> These treatments move away from the interests focus, and instead consider how disciplinarity influences the knowledge claims of the scientific sub-communities. These studies consider the corporate differences that underlie the approaches of the disciplinary groups and how those differences influence the knowledge products of the communities.

In this section, I argue that a useful way of examining the role of disciplinarity in controversy is to extend boundary theory, and to an extent the cartographic metaphor, to an analysis of the internal delineations of science. This method allows the activities of advocates and adversaries from different disciplinary groups to be framed as boundary work. To enable this method the cartographic metaphor must be refined to have a further level of focus than in standard boundary work analyses. This increased level of focus can be thought of as a modification of the cartographic gaze. The order of boundary normally considered, i.e. that between science and the other non-science cultural domains like politics and art, is equivalent in the cultural cartography to a national boundary. A refined gaze, that allows a study of disciplinarity within science, visualises boundaries that are equivalent in the cultural cartography to state or county delineations.

One of the problems with the cartographic metaphor is that it implies adjacencies that are not necessarily relevant or useful. The geography of, and distance between, the delineated areas on the map do not reflect stable adjacencies or relations between areas of culture. The cartographic metaphor is necessarily illustrative rather than descriptive. It provides a visual metaphor for delineation, but it does not reflect the actual relationship between territories in terms of adjacency or overlap. We cannot begin to paste terms like 'science', 'politics' and 'art' into the blank spaces and hope to have depicted the relations between the areas of culture. To

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<sup>575</sup> Gieryn, T. (1999a)

<sup>576</sup> Gieryn, T. (1999a)

understand the relationship between territories the cartographic metaphor has to become the multiple maps metaphor, in which there is no one constant map of culture, only maps that emerge from the way the delineations are looked at. For example, from some perspectives chemistry and physics would be adjacent on the map, overlapping even, and comparatively friendly. However, under some circumstances, i.e the cold fusion debate that I discuss below, the delineation of these territories becomes sharply defined, they act as islands and are at war with one another. So cartographies are as local and episodic as the territory delineations that they provide a metaphor for.

To avoid the confusion that stems from misleading adjacencies, I suggest that it might be helpful to use a more abstract graphic to depict the internal delineations of science. For example:

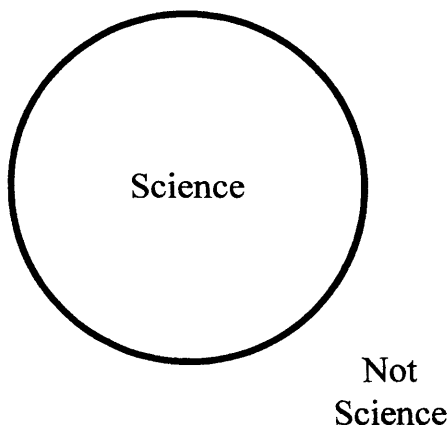


Figure 2: An abstract depiction of the territory 'science'

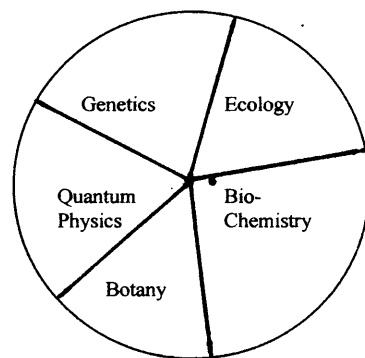


Figure 3: An abstract depiction of a few of the disciplinary delineations within the territory 'science'

The benefit of using this circular graphic of delineation is that it allows notions of orthodoxy/unorthodoxy and centre/periphery to be included in the visualization of the internal space of science. For example the most orthodox and mainstream work in any discipline would be located on this abstract map close to the centre. The more

unorthodox work is closer to the periphery. Some work or theories exist at the edges and are marginalized, being on the verge of being rejected from the territory 'science'. The graphic therefore allows for the fact that even within the specific disciplines that comprise science there is not absolute consensus regarding theoretical frameworks. There is heterogeneity within disciplines caused by the fact that the various practitioners might subscribe to a variety of different schools of thought. Furthermore, as Martin Rudwick has observed in relation to the Devonian controversy, the territories delineated within science do not necessarily 'enclose an undifferentiated interior of equal competence'.<sup>577</sup> Therefore, when attention is turned to scientific disciplinarity, and contests between disciplinary groups, there remains a considerable amount of heterogeneity in practice and competence to be taken into consideration.

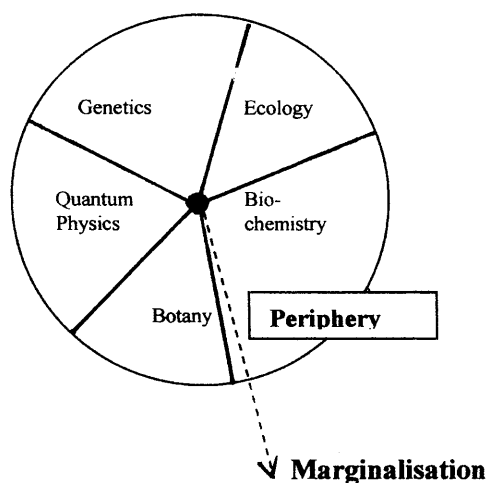


Figure 4: Notions of centre, periphery and marginalization can be built into the abstract depiction of the interior of the territory science.

In the case of the directed mutation debate, the site of contest is at the delineation that separates evolutionary biology and molecular biology. The conflict

<sup>577</sup>Rudwick, M. (1985) p.419

between evolutionary biologists and molecular biologists was the result of a contest for authority between the two communities. The directed mutation researchers had made statements about the implications of a bacterial anomaly for evolutionary theory. Essentially, they had attempted to extend their authority to make statements about issues normally outside the domain of their discipline. The evolutionary biologists perceived those statements as an infringement, and attempted to reassert their singular authority to describe evolutionary phenomena. The first publications on directed mutation show this disciplinary boundary dispute emerging.

Sociologist James McAllister has argued that, to make a contribution to SSK, it is not enough to describe the tension between disciplines. Analyses must go further and consider the extent to which that tension influences the nature of the knowledge claims that emerge from the different disciplinary groups.<sup>578</sup> In the case of the directed mutation debate, we can see how the boundary motivations of each of the communities are reflected in their approach to the knowledge claim. The activities in the early part of the debate are very similar in nature to those that McAllister has observed during the disciplinary contest of the Cold Fusion debate (see 4.1.2). For example, the molecular biologists are enthusiastic about the prospects for a 'strong version' of directed mutation theory. They assert theories that include environment direction of very specific mutations, and view those theories as very much in conflict with Neo-Darwinism and the central dogma. For the molecular biologists, the stronger the version of directed mutation the more impact it has on evolutionary biology. In turn, the more influence the anomaly has on evolutionary biology the stronger their case for an extension of their authority into the domain usually exclusive to evolutionary biologists.

Meanwhile, the evolutionary biologists were interested in reducing or eliminating the impact of the anomaly on their discipline. It was in their interest to eradicate the anomaly, and so with it eradicate the material of the molecular biologists' challenge to the boundaries of their discipline. For that reason they employed two tactics in their response. First, they attempted to attribute the phenomenon to error or poor methodology. Second, they offered explanations that

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<sup>578</sup> McAllister, J. (1992)



allowed for the existence of the observations, but did so within a framing that made them non-Lamarckian, and not anti-Darwinian. Those authors (i.e. Stahl, and later Lenski and Mittler) offered a weak model of directed mutation that allowed for a special mutational process, specific to bacteria and existing as a stress response to challenging environments. The evolutionary biologists did not go as far as to attempt to discredit the directed mutation researchers, but they did imply they that had misinterpreted the significance of their observations for evolutionary theory.

McAllister suggests that disciplinary contests have the effect of (i) colouring participants' responses with professional interests, (ii) encouraging corporate responses on the part of participants, and even (iii) influencing the very nature of the knowledge claims that contestants present. We see this borne out in the directed mutation debate. Firstly, the advocates' mode of presentation was selected in service to their professional interests of expanding their authority. They not only reported an anomaly, but also asserted its challenge to evolutionary biology. Meanwhile, their adversaries' criticisms served their interest of protecting their existing authority. Secondly, evolutionary biologists banded together in their criticism, forming a united front (particularly Levin, Lenski and Mittler), acting in corporate response to the threat. As biologist Billy Goodman has put it: 'Cairns' article galvanised evolutionary biologists'.<sup>579</sup> Thirdly, the nature of the advocates knowledge claim was influenced by the disciplinary authority contest – being framed as Lamarckian, where without that contest it might just have passed as a bacterial anomaly (see below for contrast, Hall who made similar observations but reported them without framing an authority challenge).

Bound up with the legitimacy of the directed mutation claims was the issue of which disciplinary group possessed the authority to describe the phenomenon. Their motivations regarding that authority struggle in part determined their activity during the debate.

#### 4.1.2 Molecular biology versus evolutionary biology: a conflict of 'styles' of science.

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<sup>579</sup> Goodman, B. (1992) Directed mutations: heredity made to order. *Mosaic*, 23(1): 24-33

In this section, I argue that the different approaches of the molecular biologists and the evolutionary biologists are also partly attributable to underlying differences in 'style' between the two groups. I attempt to characterise these sub-disciplines, taking into account differences in their methodology, knowledge base and growth as a way of contrasting their nature and practice. I suggest that these differences have made it difficult for the two groups to carry out effective negotiation. I describe their different approaches using the language of Kuhn's 'paradigms' and Lakatos's 'research programmes'. I argue that molecular biology encourages that its participants act as a research school, while the nature of evolutionary biology encourages a tendency towards paradigm defence.

To establish a characterisation of the two disciplines it is necessary to start by making some general statements about what distinguishes the two groups styles.

### Evolutionary Biology

This branch of science is cohered by its participants' consensus regarding the broad utility of Darwinian theory as a tool for revealing the processes and history of evolution. The discipline is convened around that 'one big theory' - and a range of laws, or dogmas, have been created which validate and protect its centrality. For example the ubiquity of Survival of the Fittest ensures the action of Natural Selection, and The Central Dogma ensures that the material of Natural Selection is random with respect to the demands of the environment. Adaptation is central to interpretations within evolutionary biology. Evolutionary biologists are engaged in a project to submit more and more elements of the natural world to analysis through the adaptionist programme of Neo-Darwinism. They take new cases and use their 'one big theory' to create explanations and new knowledge claims regarding evolutionary history. The new material examined can be from any level of nature; from the microbiological, to macro-evolutionary phenomena of tempo and mode in evolution. Biologists Stephen Jay Gould and Richard Lewontin have described this

work as ‘evolutionary story-telling’ and stated that it dominates activity in evolutionary biology.<sup>580</sup>

The theoretical basis of evolutionary biology is also dignified by a ratifying history of its success and triumph over other theories, and is consolidated by stories of a founding father who is characterised as a ‘genius’ (see Chapter 3). Much rests upon consensus regarding the value of the discipline’s key theory, and as a result there is very little heterogeneity within the discipline in terms of alternative knowledge claims or beliefs regarding the processes of evolution. The body of knowledge that has been created by the application of Darwinian theory through the adaptionist programme is open to little review.<sup>581</sup> The theory is assumed correct and therefore its interpretations carry authority. Attempts from within the community to alter tenets of the theory are not welcomed.<sup>582</sup> The result is that while new knowledge claims are added through the application of the theory to more cases, other claims are not being replaced or revised.

Overall, the activity in evolutionary biology might be characterised best using the familiar language of Kuhn’s ‘paradigm’. Kuhn suggested that paradigms determine what is to be examined, how things should be examined, and what kind of interpretations should be made within a discipline.<sup>583</sup> This fits the disciplinary activity that I have described in this characterisation. In service to that paradigm evolutionary biologists carry out ‘paradigm defence’, protecting their ‘one big theory’ from anomalies and challenges by rejecting them or negating them, and declaring them not serious enough to demand a revision of the paradigm. The

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<sup>580</sup> Gould, S. J. & Lewontin, R. C. (1979) The spandrels of San Marco and the Panglossian Paradigm: a critique of the adaptionist programme. *Proceedings of the Royal Society of London, Series B*, 205 (1161): 581-598.

<sup>581</sup> Steven Jay Gould challenged the adaptionist programme in evolutionary biology. He argued that not every trait and behaviour in the natural world should be submitted to ‘story-telling’ to reveal its probable adaptive value. Gould declares that it is a troubled methodology in which ‘plausibility alone is a criterion for accepting speculative tales’ [Gould & Lewontin (1979) p.581]. Gould’s critique was not well received and was challenged robustly; in particular, by Richard Dawkins, a keen adaptionist. See Sterelny, K. (2001) *Dawkins vs Gould: survival of the fittest*. Icon Books, Cambridge; Brown, A. (1997)

<sup>582</sup> For example Niles Eldredge and Steven Jay Gould, who attempted to explain gaps in the fossil record not as missing items that are yet to be found, but rather, as evidence of rapid spells of evolution interspersed with long periods of stasis. This conflicts with the Darwinian tenet of ‘gradualism’ in evolution. [see: Eldredge, N. & Gould, S. J. (1972); Eldredge, N. (1989) *Time frames: the evolution of Punctuated Equilibria*. Princeton University Press, Princeton, New Jersey.]

<sup>583</sup> Kuhn, T. (1962)

success of evolutionary biologists in protecting the Neo-Darwinian paradigm is determined by their success at limiting dissent. Thus, we can see one goal of the evolutionary biologists' criticism of directed mutation as the defence of the Neo-Darwinian paradigm. The authority of evolutionary biology is necessarily linked to the endurance and continuation of the Neo-Darwinian paradigm – essentially because evolutionary biology is Neo-Darwinian theory. Thus boundary work for the protection of authority leads evolutionary biologists to defend the paradigm which convenes their authority.

### Molecular Biology

In molecular biology practices are rather different. Rather than having a single theory acting as a paradigm, molecular biology instead has a body of knowledge and an associated range of methodologies that have produced that knowledge. In place of laws, there is accumulated observation that demonstrates patterns or general rules. There is not one central issue that must be agreed upon between practitioners, other than that there is a molecular world. New work in the discipline emerges as other areas of the molecular world are scrutinised, examined using any of numerous methodologies, and described in an agreed language. New claims have to come from experiments, rather than from predictive story telling based on theory. There is reasonable internal heterogeneity, which does not cause fundamental problems since there is no major impetus for complete consensus. Individual knowledge claims do not necessarily carry huge importance, since they do not directly reflect upon the authority of the method that created them. In molecular biology lots of new claims are constantly added, and lots are revised. Those revisions occur without undermining the disciplinary practice. This is much less at stake in molecular biology, since contests do not demand defence of one central theory.

The language of Lakatos' research programmes helps us contrast the activity in molecular biology with that in evolutionary biology. Lakatos' model views the scientific enterprise as marked by: growth, the discovery of new facts, the

development of new techniques and the achievement of more precise predictions.<sup>584</sup> Lakatos suggests that research programmes have a 'hard core' of theory (negative heuristics), which is surrounded by an 'auxillary belt' of hypotheses (positive heuristics). This belt of hypotheses protects the hard core, with objections and conflicts dealt with as positive heuristics. Successful research programmes grow over time, those that are unsuccessful degenerate and their hard core is falsified. For Lakatos, the 'work' in research programmes is the processing of the auxiliary belt hypotheses. It is a cumulative task to which all workers contribute. Research programmes proceed through 'problem shifts' – not requiring any 'revolution' to change course, but rather a shift of the heuristics that define the research programme.

A key difference between the Lakatosian model and the Kuhnian is in the way that anomalies are dealt with. In paradigm theory any anomaly represents a threat to the paradigm and must be either rejected or redefined. Otherwise, the paradigm must be abandoned and a 'revolution' must occur. In the Lakatosian model, the hard-core is protected from conflict and negotiation, and need only come under threat if the programme itself begins to degenerate. Lakatosian type practice reflects the details of the characterisation of molecular biology that I have offered. Of course these traits are not entirely absent from evolutionary biology, but in that discipline they are the outcome of a practice that serves a single theory, whereas in molecular biology these traits define the practice.

These relatively unarticulated differences in practice between the disciplines makes them badly suited to engage together in negotiation. In the case of the directed mutation debate this difference in styles led to each discipline approaching the other in a way that was quite inappropriate. For example, the molecular biologists set about launching an assault on the central paradigmatic premise of evolutionary biology, creating a most offensive situation from the perspective of evolutionary biological practice. They launched an assault that had a significance beyond anything that could happen within their own discipline. Meanwhile, the evolutionary

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<sup>584</sup> Lakatos, I. (1970)

biologists demonstrated a similar lack of understanding of appropriate practice in molecular biology by attempting to refute the new claims by recourse to precedent from dogma. Molecular biologists were subjected to a stifling of their interpretive flexibility.

The result was that evolutionary biologists were violently opposed to the implications of directed mutation and wanted the anomaly to be reinterpreted as unproblematic, while the molecular biologists felt that over-strong pressures of proof had been placed upon them by a community that used empty precedent as refutation. The molecular biologists were asking that the anomaly be accepted, but for the evolutionary biologists its assault on the Neo-Darwinian paradigm prevented that. Thus, the styles of the groups meant they did not know how to cope with the other's approach, or necessarily how to understand the contest in the context of the others disciplinary practice. For the molecular biologists closure would mean the synthesis of these bacterial observations into the body of knowledge, or their rejection as incorrect. For the evolutionary biologists closure would mean refuting the existence of any anomaly, or reinterpreting the anomaly such that it came within the explanatory power of their paradigm. The result was that the two groups did not even share an idea of how this debate might end, let alone how that might be achieved.

This difference in styles is reinforced by the different intellectual challenges of molecular biology and evolutionary biology, and by the training of specialists in each field. Molecular biologists' desire to share authority regarding the right to describe evolutionary phenomena did not suddenly confer upon them an understanding of or training in evolutionary biology. As Paul Rainey has pointed out:

'Despite the fact that molecular biologists confront the stuff of evolution on a daily basis, our understanding of this process is at best fuzzy and at worst just plain

wrong! Unfortunately there is nothing new here and the situation is unlikely to improve without a rethink of the way undergraduate microbiologists are trained.<sup>585</sup>

Thus the appeal for expansion by any discipline amounts to an application to share authority without necessarily sharing practice. In the case of evolutionary biology this is a further threat. One aspect of evolutionary biology that gives it integrity and authority is that it is practiced by evolutionary biologists. If others begin to engage, and introduce their own practice, then evolutionary biology ceases to be a thing in itself, and instead becomes a branch of study in various other science disciplines. This fact makes this contest even more problematic.

#### 4.1.3 An analogous case: chemistry versus physics in the cold fusion debate.

On March 23<sup>rd</sup> 1989 electrochemists Martin Fleischmann and Stanley Pons held a press conference at the University of Utah to announce their discovery of 'cold fusion'. They stated that palladium metal immersed in a heavy water solution, under electrostatic pressure, could generate four times the energy required to set the process in motion. Essentially, they claimed to have discovered a safe, clean process by which to generate energy.<sup>586</sup> They had made thermal measures of the excess energy released, and had detected neutrons, tritium and helium, implying a nuclear fusion process underlay the energy production.

Inevitably, their announcement attracted huge attention from the media, the public and scientists. Not only was the promise of this new way of producing energy of great practical importance, but also it implied an entirely new interpretation of the physics of fusion. In addition, the announcement of the discovery through a press conference was highly unusual, and appeared controversial to the scientific community. Pons and Fleischmann had bypassed the standard professional controls of peer review and journal publication, choosing instead to make sensational claims

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<sup>585</sup> Rainey, P. (2004) Bacterial populations adapt - genetically, by natural selection, even in the lab! *Microbiology Today*, 31 (Nov): 160-162.

<sup>586</sup> Simon, B. (2002)

directly to the popular media.<sup>587</sup> Immediately after the press conference, attempts to replicate the results began internationally, and a furious debate concerning their legitimacy was set in motion. However, it was not only the nature and replicability of the results that caused dissent. The Utah announcement had also precipitated an interdisciplinary clash between chemists and physicists.

Pons and Fleischmann were chemists, yet the material of the cold fusion claims was grounded in physics. Not only had they ‘published’ outside their narrow, specialist electrochemistry field, but they had done so without first consulting colleagues with special skills in physics or nuclear physics. Furthermore, the physical process that their results implied went against tenets of energy physics. Both the material and the style of their announcement were controversial, and transgressed limits of theory and practice. The greatest contention rested upon the fact that a pair of chemists claimed to have made a discovery of exceptional importance in the field of physics.

The majority of objections came from nuclear physicists, who argued that it was unlikely that a chemical process of any kind could produce a nuclear reaction, and that it would certainly not exist to a degree that would allow any commercial production of energy.<sup>588</sup> Most critics suggested that Pons and Fleischmann’s results were the outcome of poor interpretation or experimental error. The replications attempted worldwide did not resolve the issue. Some reports emerged supporting the claims, while others suggested that no such effect existed. Some critics went as far as to make accusations of fraud, others settled for a verdict of incompetence and poor professionalism.<sup>589</sup>

Only one month after the Utah press conference Pons and Fleischmann appeared before congress to request \$25 million for research into cold fusion.<sup>590</sup> Effectively, congress was being asked to decide the credibility of the cold fusion research, a highly unusual situation in the negotiation of scientific authority.<sup>591</sup>

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<sup>587</sup> Gieryn, T (1999a)

<sup>588</sup> Simon, B. (2002)

<sup>589</sup> Simon, B. (2002)

<sup>590</sup> Peat, D. (1989) *Cold fusion: the making of a scientific controversy*. Contemporary Books, Chicago.

<sup>591</sup> Gieryn, T. (1999a)



Witnesses were called to attest to the viability of Cold Fusion. The clash between chemists and physicists is reflected in the statements collected. Pons and Fleischmann placed emphasis upon the implications for national economics and safer power production. Conversely, when Steve Jones, a physicist from Brigham University, was called as a witness he emphasised how far away the observations were from being transformed into a usable technology, and added that quite possibly an application might never emerge. Jones pointed out that his team had made similar observations, but had not published because the results were far from being confirmed, and further from being applied as a technology.<sup>592</sup> He implied that Pons and Fleischmann's announcement had been premature. While the chemists emphasised the applications of cold fusion, the physicists insisted that science must come before technology. Ultimately, congress offered no funding. It seemed they favoured Jones' argument that facts must precede speculation.<sup>593</sup> Nevertheless, Pons pursued Cold Fusion at the American Chemical Society (ACS) in April 1989, where 7,000 delegates heard him announce that chemists had 'saved the day'.<sup>594</sup>

In May 1989 the American Physical Society (APS) met and physicists negotiated the claims without inviting the press. They sought to reintroduce professional rigour into the sensationalised debate.<sup>595</sup> Pons and Fleischmann did not attend despite being invited. Gieryn states that the APS meeting was the site at which Pons and Fleischmann were constructed as object lessons regarding the transgression of scientific norms, and were pushed to the periphery of orthodox science.<sup>596</sup> At the APS conference the physicists reasserted the boundaries of physics, which had been challenged by Pons and Fleischmann. To limit the damage to the authority of physics, and science in general, the physicists sought to brand Pons and Fleischmann as unscientific, and even fraudulent. Gieryn and Figert have observed this method of protecting professional authority in scientists' 'status degradation' of Sir Cyril Burt.<sup>597</sup>

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<sup>592</sup> Gieryn, T. (1999a)

<sup>593</sup> Gieryn, T. (1999a)

<sup>594</sup> McAllister, J. (1992)

<sup>595</sup> McAllister, J. (1992)

<sup>596</sup> McAllister, J. (1992)

<sup>597</sup> Gieryn, T. & Figert, A. (1986)

Pons and Fleischmann had made a double transgression. First, the manner in which they had announced their work had offended against established practice. They had threatened to undermine the authority of science by introducing sensationalism and speculation into a cultural domain that is cohered by its disavowal of those tactics. Second, they had made an assault on the disciplinary boundaries of physics. Not only had they made statements that conflicted with physical laws, but they had also done so without themselves being physicists. They had attempted to assert authority in a territory of which they were not members.

The debate was short-lived, and order among the disciplines was restored. By July 1989 John Maddox wrote in a *Nature* editorial that: 'The brief spell in April when it seemed as if cold fusion would permanently divide chemists and physicists has left no trace.'<sup>598</sup> Chemists had made a challenge to the boundaries of physics, physicists had defended their territory, and the debate was deemed closed with no change to authority required.

Sociologist James McAllister has offered a thorough treatment of this clash between chemists and physicists.<sup>599</sup> He considers the conflict to be based on professional interests, and describes how disciplinarity exists as a social construct for the management of authority and funding. He argues that the cold fusion episode put at stake the corporate interests of both the physicists and chemists. For the chemists, the cold fusion 'discovery' offered to increase the public standing of their discipline, and increase the authority allocated to their community. This would result in the fulfilment of interests related to the acquisition of funding, since money would be directed towards chemistry communities for the investigation of cold fusion and the development of technologies. For physicists, the emergence of cold fusion from an alternative disciplinary context threatened to undermine the authority of their specialism, and disrupt the boundaries that cohere their community. It also threatened loss of finances, since money for fusion research would be redirected towards chemistry and away from the physics.

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<sup>598</sup> Maddox, J. (1989c) End of cold fusion in sight. *Nature*, 340:15.

<sup>599</sup> McAllister, J. (1992)

So it was in the interests of chemists that the claims prove correct, and in the interest of the physicists that the claims be rejected. McAllister goes as far as to suggest that it was in the physicists' interests to actually discredit the chemists and their claims.<sup>600</sup> To protect their disciplinary authority it was necessary to demonstrate that only physicists are able to do fusion research, and that chemists do not have the required skills to approach the subject. McAllister suggests that the response of the physicists at the APS in 1989 demonstrates their pursuit of that goal; at that meeting they not only branded the experiments unrepeatable, but also denigrated the methodology and practice of Pons and Fleischmann.

McAllister describes how the disciplinarity of the groups involved in the Cold Fusion debate determined their relationship with and reaction to the knowledge claims of Pons and Fleischmann.<sup>601</sup> In the months immediately after the Utah press conference the advocates and adversaries were collected in three main bodies; the APS, the ACS and the Electrochemical Society. McAllister argues that the members of each discipline showed solidarity under the disciplinary banner of these organisations, showing their corporate reaction to the challenge. At the ACS and Electrochemical Society meetings the mood was positive and repetitions with positive outcomes were presented. Conversely, at the APS negative replications were reported, methodological problems were emphasised and the technological aspirations of the chemists were dismissed. Both groups were examining the same anomaly, and both had access to the same reports. However, the disciplinary associations of each group encouraged them to construct the situation in very different ways, both of which supported the interests of their particular discipline. Their approaches to the knowledge claim existed as a tool of their disciplinary professionalisation in this debate, and thus of the boundary work for either expansion or protection.

#### 4.1.4 How the clash of sub-disciplines perpetuates negotiation.

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<sup>600</sup> McAllister, J. (1992)

<sup>601</sup> McAllister, J. (1992)

In the case of the cold fusion debate it seems that the clash of sub-disciplines resulted in the fairly rapid closure, although, that closure was achieved by discredit of the Utah claims, rather than by the achievement of consensus regarding the validity of the cold fusion claims. The scientific mainstream rejected cold fusion, following the lead of the physicists, who declared the debate resolved in their favour. However, as Bart Simon points out in his analysis of 'the afterlife' of cold fusion, not a great deal changed in terms of the amount of support for the theory and research into the phenomenon as a result of the declarations of closure.<sup>602</sup> The same is true of the directed mutation debate. By the mid-1990s evolutionary biologist Daniel Dennett felt confident to proclaim that directed mutation debate was over and that the theory had been safely discredited.<sup>603</sup> However, those assertions of closure didn't indicate a decline in interest in the phenomenon. In the case of directed mutation the assertion of discredit didn't even signal an end to journal publications on the issue.

So, although interdisciplinary authority struggles might encourage statements that imply closure has been achieved, the reality is quite different. The clash of disciplines actually serves to perpetuate controversy. The involvement of a disciplinary clash in a scientific contest results in an increase in the significance of the outcomes of the contest. Both sides in the debate are pursuing outcomes/closures that serve the interest of their community, and each disciplinary group acts as a corporation in the pursuit of their goals related to the extension or protection of authority. As the groups negotiate there is more at stake than the acceptance or the rejection of the contested knowledge claim. For the group that is attempting to extend its authority into another discipline's territory the perpetuation of the debate becomes an interest in itself. The greater life expectancy they can give to their challenge, through their advocacy, the greater their chance of success. Conversely, the group engaged in protection of their authority is interested in limiting the life expectancy of the challenge. They therefore use closure rhetoric and attempt to dampen the impact of the challenge. How each group positions itself in the contest is

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<sup>602</sup> Simon, B. (2002)

<sup>603</sup> Dennett, D. (1995)

related to more than the material of the knowledge claim itself. As McAllister has pointed out, a group's very reaction to, framing of and position on a particular claim can be motivated as much by disciplinary interests, as by the nature of the scientific claim itself.<sup>604</sup>

In the case of both cold fusion and directed mutation it is possible to hypothesise that the debates might have been conducted differently had they not incorporated this element of disciplinary competition. McAllister has attempted to show this by comparing the case of cold fusion to a similar debate (the ZETA episode<sup>605</sup>) that did not involve the clash of disciplines. It is problematic to use that kind of methodology to assert that the outcome of a debate might have been different, since it implies an asymmetrical and present-centred approach. However, I suggest that it is feasible to make the case that controversies might be more quickly resolved without the clash of disciplines being involved. With the clash of disciplines come further degrees of dissent than are manifest in uni-disciplinary contests. Essentially, a contest is 'worsened' when the material of the debate comes to include professional interests, stake-holdings, and boundary work. There are many more points of conflict in those kinds of debates than in cases where the dissent is confined to the material of an anomaly.

Debates are also protracted by the different ways that groups practice. Whereas the protraction caused by disciplinary interests is partly a conscious tactic, the perpetuating factors that arise from the different practices between disciplines are more subtle and accidental. Each of the groups in a disciplinary clash might work with totally different relationships to anomaly, as is the case in the comparison of molecular biologists and evolutionary biologists. Their different approaches mean they have different identities of the significance of anomaly, and different notions of

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<sup>604</sup> McAllister, J. (1992)

<sup>605</sup> In this case, the contested claim also related to the generation of energy by a controlled fusion process. It centred around apparatus in use at the Atomic Energy Research Establishment in the UK. In 1957 it was claimed that the Zero Energy Thermonuclear Assembly (ZETA) had generated excess energy during a fusion experiment. This claim arose from within the nuclear physics community, and was contested, negotiated and resolved within that community between 1957-1958. It was concluded that the appearance of fusion had been an artefact, but as McAllister points out that resolution was achieved without any 'bad-tempered controversy' and without either the advocates or adversaries in the debate casting aspersions regarding the other's practice. (McAllister, 1992 p. 39)

how to negotiate and resolve observations of anomaly. These differences in approach can be significant enough that the groups involved in an attempt to close a conflict might not share enough common ground to recognise the same outcomes as closure, let alone agree about how negotiation might best direct resolution.

For the molecular biologists directed mutation in bacteria can be added to their knowledge complex. The addition of the claim does not jeopardise other areas of their knowledge, and it can be synthesised. For the evolutionary biologists, the claim must be rejected so as to protect the tenets that cohere their discipline. To accept the claim would require a major revision of existing knowledge. The stakes are effectively much higher in evolutionary biology. The terms of debate are different for each group, and so debate is stifled at its very premise. Both groups engage in what they see as negotiation – one group seeking synthesis, the other seeking rejection – but in effect they have no shared goal in terms of the closure they anticipate.

The negotiation becomes protracted as the molecular biologists proceed from observations to theory in service to their agenda. They focus on speculative theories as a way of achieving potential resolution. They try to explain the mode of directed mutation, and attempt to negotiate that topic with the evolutionary biologists. Meanwhile, the evolutionary biologists are less interested in mechanism than in eradicating the challenge to their discipline. Although they demand details of mechanism from the molecular biologists, as I have described earlier, this is more a rhetorical device to assert pressure across the disciplinary divide. Evolutionary biologists have existing assumptions that refute directed mutation, for example the central dogma and the fluctuation test. While the directed mutationists push for synthesis, the evolutionary biologists remind them of this precedent. So, the groups appear to be negotiating, but are in fact talking at each other, rather than to each other; closure becomes a remote possibility.

#### **4.2 Dedicated advocacy perpetuates negotiation: introduction.**

In this section Cairns' role in the directed mutation debate is considered, and his dedicated advocacy is highlighted as one of the principal perpetuating forces in this conflict. I describe his contributions to science through a short biography, and highlight the two controversies in which he has been a central character. I describe Cairns' self-aware role as an advocate, and consider the interests and motivations underlying his advocacy. I use boundary work methodology to explore Cairns' Lamarckism, arguing that this unorthodoxy might be seen as a tool of advocacy. I contrast Cairns' advocacy with that of Barry Hall, portraying the two authors respectively as 'loud' and 'quiet' advocates. Finally I describe the roles of analogous 'loud' and 'quiet' advocates in the Cold Fusion debate.

#### 4.2.1 John Cairns: A short biography<sup>606</sup>

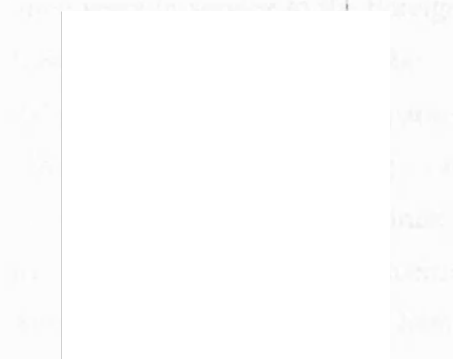


Figure 5: John Cairns at the Max Delbruck Harbor Laboratory dedication at Cold Spring Harbor in 1981.



Figure 6: John Cairns at the 1978 Cold Spring Symposium on Quantitative Biology.

Hugh John Cairns, or John Cairns as he is better known, was born in Oxford, England on November 21<sup>st</sup>, 1922. He was named after his neurosurgeon father Sir Hugh Cairns who had achieved a degree of academic and professional celebrity as the first Professor of Surgery at Oxford's Radcliffe Infirmary. He attended Edinburgh Academy from 1933 to 1940, and then moved to Balliol College, Oxford

<sup>606</sup> The details of this biography have been constructed from personal communication with John Cairns.

where he attained a BA degree in 1943.<sup>607</sup> After graduation, Cairns followed in his father's footsteps and studied medicine at Oxford University. He attained his MD qualification in 1946. As a medicine graduate he worked first as a House Physician at the London Postgraduate Medical School from 1946-1947 and then, for the remainder of 1947, at the department of paediatrics at the Royal Victoria Infirmary, Newcastle. Between 1947 and 1949 Cairns returned to the seat of family celebrity and worked as a clinical pathologist at the Radcliffe Infirmary in Oxford, undertaking technical work in cancer biology.<sup>608</sup> In 1948 he married Elspeth Forster, and changed his name to Hugh John Forster Cairns.

Cairns spent the following two years at the Walter and Eliza Hall Institute in Melbourne, Australia, where he worked on influenza virus replication with the support of British Foreign Office funding. He was subsequently obliged to work for three years in service to the Foreign Office and so, in 1952, took a post at the Virus Research Institute in Entebbe, Uganda.<sup>609</sup> In Uganda he studied numerous indigenous diseases and local parasitic conditions, and became interested in the viral and DNA factors underlying some forms of cancer. He began to make key contributions to the study of viruses, being the first to describe the reproductive and lytic release cycle of the influenza virus in 1952<sup>610</sup>. In 1955 Cairns returned to Australia, where he joined the John Curtin School of Microbiology at the Australian National University. He continued his work on viruses, focussing on influenza, vaccinia and bacteriophage. In 1957 a fellowship from the Rockefeller Foundation allowed Cairns to spend a four month sabbatical at the California Institute of Technology learning the tissue culture of viruses from Renato Dulbecco who had

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<sup>607</sup> See "Cairns, Hugh John Forster" in Porter, R. & Oglivie, M. (Eds.) (2002) *The Hutchinson Dictionary of Scientific Biography, Volume 1*. Helicon Publishing, Oxford.

<sup>608</sup> Cairns, J. & Lajtha, L. (1948) Loss of white cells in bone marrow culture. *Nature*, 162: 536-537

<sup>609</sup> Fenner, F. & Curtis, D. (2001) John Cairns and autoradiography. In: *The Curtin School of Medical Research: the first fifty years, 1948-1998*. Brolga Press, Australia.

<sup>610</sup> Cairns, J. & Edney, J. M. (1952) Quantitative aspects of influenza virus multiplication. I. Production of the incomplete virus. *Journal of Immunology*, 69: 155; Cairns, J. (1952b) Quantitative aspects of influenza virus multiplication. III. Average liberation time. *Journal of Immunology*, 69: 168; Fazekas de St. Groth, S., & Cairns, J. (1952) Quantitative aspects of influenza virus multiplication. IV. Definition of constants and general discussion. *Journal of Immunology*, 69:173; Cairns, J. & Mason, P. J. (1953) Production of influenza A virus in the cells of allantois. *Journal of Immunology*, 71:38.



developed the technique with his colleague Marguerite Vogt.<sup>611</sup> In California he lived with fellow molecular biologists Jan Drake, Matthew Meselson<sup>612</sup> and Howard Temin<sup>613</sup>, in a house that was ‘in the throes of the N15-N14 transfer experiment that was to settle the semi-conservative nature of the replication of bacterial DNA’<sup>614</sup>.

Back at the John Curtin School, in 1959, Cairns reported the first of his better-known achievements, the first genetic mapping of an animal virus.<sup>615</sup> In 1960, while on a year long National Institutes of Health (NIH) funded sabbatical from the Australian National University, Cairns worked in Alfred Hershey’s laboratory at Cold Spring Harbor and added to his virology achievements by making the first measurement of the T2 bacteriophage DNA molecule.<sup>616</sup> That experiment further confirmed the double stranded structure of DNA that Watson and Crick had

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<sup>611</sup> Dulbecco’s development and teaching of the tissue culture techniques is considered to have revolutionised the study of viral agents. The tuition that Cairns received from Dulbecco enabled much of his subsequent research in virology. For details of Dulbecco’s tissue techniques see: Kevles, D. (1993) Renato Dulbecco and the new animal virology: medicine, methods and molecules. *Journal of the History of Biology*, 26(3): 409-442 and for primary sources: Dulbecco, R. & Vogt, M. (1954) Plaque formation and the isolation of pure lines with poliomyelitis viruses. *Journal of Experimental Medicine*, 99:167-182; Dulbecco, R. (1966) The plaque technique and the development of quantitative animal virology. In: J. Cairns, G.S. Stent & J. D. Watson (Eds.) *Phage and the origins of molecular biology*. Cold Spring Harbor Laboratory Press, New York.

<sup>612</sup> In 1958 Mathew Meselson, along with Franklin Stahl, demonstrated that bacterial DNA replicates by the splitting of the DNA helix, and the replication of a new second strand using the original as a pairing template. This is the mode of replication that Watson and Crick had predicted when they described the helical structure [Watson, J. & Crick, F. (1953) The structure of DNA. *Cold Spring Harbor Symposium on Quantitative Biology*, 18: 123-131]. Meselson and Stahl showed, using radioactive marking, that each newly generated DNA helix is comprised of one strand drawn from the parental DNA and one complementary strand synthesised from the appropriate nucleotide bases. [Meselson, M. and Stahl, F. (1958) The replication of DNA in *Escherichia coli*. *Proc. Natl. Acad. Sci.*, 44: 671-682.] John Cairns later called their transfer experiment ‘the most beautiful experiment in biology’ [see Judson, H. (1996) p.163]. In the 1980s and 1990s Franklin Stahl wrote a series of important reviews of directed mutation (see Chapter 2). These include: Stahl, F. (1988); Stahl, F. (1990); Stahl, F. (1992)

<sup>613</sup> Howard Temin went on to identify the retroviral enzyme ‘reverse transcriptase’ that enables the transcription of RNA into DNA. He co-discovered the enzyme with David Baltimore, and in 1975 the two men, and their supervisor Renato Dulbecco, shared a Nobel Prize for their work. The existence of reverse transcriptase undermines the central dogma of molecular genetics, which relies upon unidirectional information transfer from DNA to protein (see Chapter 3 ). [Temin, H. & Mizutani, S. (1970); Baltimore, D. (1970)]

<sup>614</sup> Cairns, J., Stent, G & Watson, J. (Eds.) (1966) *Phage and the origins of molecular biology*. Cold Spring Harbor Laboratory Press, New York. p.252-253

<sup>615</sup> Gemmell, A. & Cairns, J. (1959) Linkage in the genome of an animal virus. *Virology*, 8, 381-383.

<sup>616</sup> Cairns, J. (1960) The initiation of vaccinia infection. *Virology*, 11: 603; Cairns, J. (1961) An estimate of the length of the DNA molecule of T2 bacteriophage by autoradiography. *Journal of Molecular Biology*, 3: 756-761; Cairns, J. (1962b) A minimum estimate for the length of the DNA of *Escherichia coli* obtained by autoradiography. *Journal of molecular biology*. 4: 407-409; Cairns, J. (1962c) Proof that the replication of DNA involves separation of the strands. *Nature*, 194: 1274.

described in 1953<sup>617</sup> and further illustrated semi-conservative replication of the kind that Meselson and Stahl<sup>618</sup> had recently demonstrated.<sup>619</sup> This was followed in 1963 by the release of his celebrated ‘autoradiographs’ of the bacterial plasmids of *Escherichia coli*.<sup>620</sup> These ‘photographs’ showed for the first time the circular cytoplasmic DNA of bacteria that would later become the foundation of recombinant DNA technology. The virology work that Cairns undertook while at the John Curtin School is cited as the basis for his election as a Fellow of the Royal Society in 1974.<sup>621</sup>

In 1963 Cairns left Australia for America, and his earlier achievements helped him gain the prestigious and high profile post of Director of the Cold Spring Harbor Laboratory of Quantitative Biology, which he held until 1968. During Cairns’ tenure the Cold Spring Harbor Laboratory was officially created, from a merger between the Long Island Biological Association’s Biological Laboratory and the Carnegie Institution of Washington’s Department of Genetics. He led the institute through what is considered to have been ‘the time of its greatest financial insecurity’; a commitment that left his family ‘with no house and with capital of only a few thousand dollars’.<sup>622</sup> At Cold Spring Harbor Cairns was a member of the ‘phage group’, a group of geneticists that included Max Delbruck, Salvador Luria, Alfred Hershey and James Watson amongst others.<sup>623</sup> In 1966 Cairns, Watson and

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<sup>617</sup> Watson, J. & Crick, F. (1953)

<sup>618</sup> Meselson, M. and Stahl, F. (1958)

<sup>619</sup> Cairns, J. (1962b) Cairns, J. (1962c)

<sup>620</sup> Cairns, J. (1963a) The bacterial chromosome and its manner of replication as seen by autoradiography. *Journal of Molecular Biology*, 6:208-213; Cairns, J. (1963b) The form and duplication of DNA. *Endeavour*, 22:141; Cairns, J. (1963c) The chromosome of *Escherichia coli*. *Cold Spring Harbor Symp. Quant. Biol.* 28, 43-46. Cairns use of autoradiographic techniques was often innovative and represents his central methodological contribution to the phage group at Cold Spring Harbor during the 1960s. See for his discussion of the development of the techniques [Cairns, J. (1962a) The application of autoradiography to the study of DNA viruses. *Cold Spring Harbor Symposia on Quantitative Biology*, 27: 311; Cairns, J. (1966a) The autoradiography of DNA. In: J. Cairns, G. Stent & J. Watson (Eds.) *Phage and the origins of molecular biology*. Cold Spring Harbor Laboratory Press, New York; Cairns, J. (1966b) The bacterial chromosome. *Scientific American*, 214: 36; Davern, C., Cairns, J., De Lucia, P. & Gunsalus, A. (1967) The bacterial chromosome as a unit of structure and replication. In: H. Vogel, J. Lampen & V. Bryson (Eds.) *Organizational Biosynthesis*. Academic Press, New York.

<sup>621</sup> Fenner, F. and Curtis, D. (2001)

<sup>622</sup> Cairns, J. (1997) p.127

<sup>623</sup> The constitution and achievements of the ‘phage group’ are described in: Cairns, J., Stent, G.S. & Watson, J.D. (Eds) (1966). Historian Horace Judson [Judson, H. (1996)] describes the phage groups

Gunther Stent published collected essays celebrating the new molecular biology that had emerged from the 'phage school', in honour of Max Delbruck's 60<sup>th</sup> birthday.<sup>624</sup> In 1967 Cairns received recognition for his achievements, being made a fellow of the American Academy of Arts and Sciences.

In 1968 Cairns resigned his directorship of Cold Spring Harbor wishing to return to research, and the position was taken by James Watson. Cairns remained on the research staff of Cold Spring Harbor until 1973. In 1969 Delbruck, Luria and Hershey shared a Nobel Prize for establishing the bacteriophage as the model system for molecular genetics research, a task they had largely achieved at Cold Spring Harbor during Cairns' directorship.

In the following five years Cairns simultaneously held professorships at The State University of New York and The American Cancer Society. He was also American Cancer society Professor at Cold Spring Harbor from 1968-1973. In 1973 Cairns returned to England to assume the directorial role at the Mill Hill Laboratories of the Imperial Cancer Research Fund, London. By this time he was a highly respected and accomplished cancer biologist, virologist, molecular biologist and geneticist. At the Mill Hill laboratories he was remembered as 'a profound thinker with marvellous vision and scientific imagination'.<sup>625</sup> From 1975 Cairns produced a series of general papers on cancer, and began to address the social implications of the 'cancer problem'.<sup>626</sup> In 1978 he published *Cancer: Science and Society*, a book based on his two decades of work on cancer biology.<sup>627</sup> In 1980 he was offered a professorship in the Harvard University Department of Microbiology, and returned to America. At Harvard he became head of the School of Public Health

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as a community cohered by communication more than geography, but who were able to make their community physical at Cold Spring Harbor each year. Their aim was to investigate every aspect of bacteriophage biology and to assert the phage as the principal tool and model for molecular biological work. Judson analogises the group to the community that existed in Copenhagen around physicist Neils Bohr. Luria had been Bohr's student and Judson suggests he modelled the group on his earlier experiences in Bohr's community.

<sup>624</sup> Cairns, J., Stent, G. & Watson, J. (Eds.) (1966)

<sup>625</sup> Austaker, J. (1988) *A history of the Imperial Cancer Research Fund, 1902-1986*. Oxford University Press, Oxford.

<sup>626</sup> Cairns, J. (1975a) Mutation, selection and the natural history of cancer. *Nature*, 255: 197; Cairns, J. (1975b) The cancer problem. *Scientific American*, 233(5): 64-78.; Cairns, J. (1977) Some thoughts about cancer research in lieu of a summary. In: H. Hiatt, J. Watson & J. Winsten (Eds.) *Origins of human cancer*. Cold Spring Harbor Laboratory Press, New York.

<sup>627</sup> Cairns, J. (1978) *Cancer: science and society*. W. H. Freeman, San Francisco.

and won regard as a public health biologist. He continued to contribute to the cancer debate<sup>628</sup>, becoming a key figure in the controversy over the effectiveness of chemotherapy<sup>629</sup>, and eventually consolidated his contributions in his book *Matters of Life and Death: Perspectives on Public Health, Molecular Biology, Cancer and the Prospects for the Human Race*.<sup>630</sup> Cairns continued to write on cancer issues during his involvement in the directed mutation debate.<sup>631</sup>

At the end of 1991 Cairns retired from the Harvard School of Public Health and returned to Charlbury, Oxfordshire where he now lives. He remained Emeritus Professor of Harvard and is now also Emeritus Professor of Cancer at the Clinical Trial Service Unit at Oxford University. Cairns has two sons and a daughter.

#### 4.2.2 John Cairns' 'normal' science

Until his involvement with two controversial episodes during the 1980s John Cairns was very much an orthodox scientist; the details of his biography illustrate his firm association with the institutions and practices of orthodox science. Cairns held prestigious positions, and received many grants, reflecting his share in the cultural authority attached to science. In this section, I use the language of Robert Merton's 'norms' of science to illustrate Cairns' participation in practices appropriate to his location inside the territory 'science'. As I described in chapter 1, I intend the language of Merton's normative scheme to be revealing as descriptive tool, but I do not intend the essentialism of his scheme to figure in this analysis. I argue that Cairns fulfilled obligations of the kind that Merton has identified as 'norms', and in turn received affirmation from his community of a kind that Merton

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<sup>628</sup> Cairns, J. (1980) Bacteria as the proper subjects for cancer research (The Leeuwenhoek Lecture). *Proceedings of the Royal Society, Series B*, 208: 121; Cairns, J. (1983) An onlooker's view of the war against cancer. *Harvard Medical school Bicentennial Symposium*; Cairns, J. (1985b) The present status of the cancer problem. *Human Toxicology*, 4(3): 341-343.

<sup>629</sup> Cairns, J. (1985a) The treatment of diseases and the war against cancer. *Scientific American*, 253(5): 51-59. And, see section 4.2.3.

<sup>630</sup> Cairns, J. (1997)

<sup>631</sup> Cairns, J. (2000b) The interface between molecular biology and cancer research. *Mutational Research*, 42(2/3): 423-428; Cairns, J. (2002) Somatic stem cells and the kinetics of mutagenesis and carcinogenesis. *Proceedings of the National Academy of Sciences, USA*, 99: 10567-10570.

has described as 'rewards'.<sup>632</sup> This approach demonstrates that Cairns did not engage in unorthodoxy during the 1980s because he was by nature an unorthodox scientist. Rather, Cairns' contentious contributions in that period did not match the character of his approach earlier in his career.

Merton's four principal norms (communism, universalism, disinterestedness and organized scepticism) can be used as categories for considering Cairns' activity during his early career. First, the norm of communism enjoins a scientist to share their findings with their community and others, and we see Cairns do this through his very numerous publications. Second, the norm of universalism suggests that scientists must base their decisions and evaluations on 'pre-established impersonal criteria'. It also asks that scientists be immune to biases based on race, gender, nationality and social class. Again, there are indications that Cairns' has fulfilled this obligation; his career being international, his co-authorships numerous and diverse. Third, disinterestedness requires that scientists focus their efforts on the institutional goal of science rather than on any personal gains. Again Cairns' adherence to this norm is apparent. Many of his contributions, especially during the 1960s, were the outcomes of research that he had embarked upon to help solve some problem that was hindering progress for the community, or to confirm the work of others. For example, his measurement of the viral genome was purposed to confirm the semi-conservative replication of DNA that was the chief concern of Meselson and Stahl in that period. Also, his development of autoradiography provided a vital tool for the genetics community that would enable many other researchers successes in the coming years.<sup>633</sup> Cairns acted as a participant in the institutional project, rather than as an individual driven by personal goals. He says himself that: 'Much of the pleasure of being a scientist comes from being part of a communal effort and being able to observe at close quarters the continual evolution in our understanding of the world around us.'<sup>634</sup> Merton's norm of disinterestedness does not go as far as to demand that a scientist be altruistic. Nevertheless, Cairns' exceeds the obligations of

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<sup>632</sup> Merton, R. (1942)

<sup>633</sup> For discussion of Cairns' autoradiographic technique see: Cairns, J. (1966a)

<sup>634</sup> Cairns, J. (1997) p.xi

the norm, allowing his family finances to suffer during his directorship of the financially unstable Cold Spring Harbor Laboratory.<sup>635</sup>

Finally, the notion of organized scepticism demands that scientists adopt a position devoid of dogmatic attachment to certain ideas, and instead practice the suspension of judgement. There is no evidence that during his early career Cairns was inflexible in his views or quick to judge the outcomes of his work or others. Rather, his open-mindedness and commitment to investigation is reflected in his description of the experience of scientific work. He says: 'You wrestle with some problem for months or years and, as you do experiments and read about other people's experiments, you find that the pieces of the puzzle are magically rearranging themselves, as though you were watching a private display of some abstract form of evolution.' In this description Cairns' abstracts the researcher from the process of their research; the outcomes simply unfold by virtue of the method applied. This does not imply that he considers it appropriate for the researcher to consciously aggravate the process by applying judgement or dogma.

So it appears, at least in terms of Merton's interpretation, that Cairns fulfils the criteria of 'normal' science. Merton goes on to describe how scientists that adhere to the norms will be 'rewarded' by their community and peers.<sup>636</sup> In this regard Cairns' experiences prior to the 1980s are also in keeping with Merton's scheme. For example, Cairns' received financial rewards; he was in constant employment and was the recipient of several grants and fellowships. At times he was in simultaneous receipt of the sponsorship of several institutions and associations. Furthermore, financial support for his work came from prestigious sources such as the Rockefeller Foundation. Cairns co-authored with several well-regarded individuals in his field, for example James Watson,<sup>637</sup> a fact that demonstrates he had gained the regard of his peers and was in receipt of the reward of association. Cairns' employment record shows some of the most meaningful 'rewards' that the scientific community has to offer. The 1960s directorship of Cold Spring Harbor was a prestige post. He also filled important and prestigious leadership roles at the

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<sup>635</sup> Cairns, J. (1997) p.127

<sup>636</sup> Merton, R. (1942)

<sup>637</sup> Cairns, J., Stent, G. & Watson, J. (Eds.) (1966)

Mill Hill Laboratories in London and at the Harvard University School of Public Health. His success at gaining these posts illustrates that the scientific community not only allocated him a share of the authority of science, but also saw him as a suitable administrator and public face of the territory. Cairns has held professorships at several universities and institutes internationally, a fact that represents both a reward and a signifier of authority from each of those institutions. Cairns received clear Mertonian-type rewards in 1967, with his election as a fellow of the American Academy of Arts and Sciences, and 1974, with his election as a fellow of the Royal Society. Throughout his career Cairns' work has achieved publication in the most prestigious journals, for example, *Nature*, *Science*, *Journal of Immunology*, *Virology*, *Genetics* and the *Journal of Molecular Biology*.

#### 4.2.3 John Cairns' unorthodoxy

In terms of boundary theory, and using Merton's norms and rewards language for clarification, it seems that at least until the 1980s John Cairns adhered to the obligations conferred by being 'inside' the territory 'science', and was rewarded by his community as a result. However, during the 1980s two episodes of controversy changed Cairns' relation to orthodoxy.

##### 1. Unpopular conclusions from four decades in cancer research

In 1985 an article by John Cairns appeared in the journal *Scientific American*.<sup>638</sup> It addressed what Cairns called the 'war against cancer', and offered an evaluation of the success of the campaign based upon Cairns' many years in cancer research. In that article, Cairns stated that chemotherapy was being vastly over-used, and that its uptake as the principal treatment for cancer was not supported by evidence that it significantly improved survival. He claimed that 'the cancer data are so discouraging that it is difficult to discuss them in public'. He suggested that aggressive treatments did not live up to their promotion, and that resources and research would be far better directed towards the improvement of screening techniques.

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<sup>638</sup> Cairns, J. (1985a)

In that paper, Cairns demonstrated statistically that improvements in screening yielded far greater increases in survival rates than aggressive chemotherapies. He also added a political angle to this dissent, arguing that the single greatest impediment to the war against cancer was governments' unwillingness to combat smoking. He attributes that unwillingness to the desire to collect taxes from cigarettes. Most alarmingly, he adds that, the British government in particular, but also the America to a degree, choose to allow smoking to continue because it decreases the amount of old age benefit they are required to pay out by shortening life expectancy. Overall, Cairns suggests that every advance that has been made in the treatment of cancer is ultimately outweighed by the US government's impotence on the smoking issue.

Cairns launches several attacks in the article. He accuses the medical profession of promoting a comparatively useless therapy, he accuses the government of worsening the cancer problem, and even suggests that governments use cancer as part of a plan to decrease the elderly population. However, the article generated surprisingly little debate. In particular, it is notable that this article was not responded to in any significant degree in scientific journals. Political scientist Michael Lerner has studied the impact of Cairns' 1985 publication as part of a broad study of the twentieth century cancer therapies debate.<sup>639</sup> He considers it interesting that Cairns' very inflammatory article did not precipitate much response from the scientific community.

To explain this lack of response, it is useful to consider Collins' and Pinch's categories of 'boundary work for rejection'. As described in Chapter 1, they suggest that rejection of an authors' work, and the possible associated removal of authority from that author, can be achieved either explicitly or implicitly, and that rejection can also be expressed in either the 'constitutive' or 'contingent' forums.<sup>640</sup> In Collins' and Pinch's language, we might think of the response to Cairns' 1985 as 'implicit rejection' in the 'constitutive forum'. That is to say, the terms of his dissent were not addressed and argued against, but rather his commentary was ignored by

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<sup>639</sup> Lerner, M. (1996) *Choices in healing: integrating the best of conventional and complementary approaches to cancer*. MIT Press, Massachusetts.

<sup>640</sup> Collins, H. & Pinch, T. (1979)



his professional peers. This kind of response has the boundary work function of marginalizing the dissent, but also represents the implicit acknowledgement by the community that Cairns had offered commentary that was not acceptable within the territory 'science'. This implicit rejection by his peers is a signifier of the withdrawal of the reward of association on this issue.<sup>641</sup>

It should be noted that response to Cairns' article in the contingent forum was quite different. In particular, engagement with the material on the Internet has been extensive. In the contingent forum the article was taken up in support of numerous groups for whom the accusations had particular significance. For cancer sufferers and their families, Cairns' framing of chemotherapy was of profound importance to their choice of care. In addition, he had sensationally attributed blame to national governments for the lack of success in the war against cancer. This evaluation was precisely the kind of evidence that practitioners of complementary cancer therapies required to recommend their alternative approaches. On the Internet, dozens of sites promoting complementary therapies for cancer reference Cairns' article as precedent for the quest for alternative treatments. In many cases, Cairns' commentary is translated into a kind of conspiracy theory; with chemotherapy framed as a tool of the medical profession for giving the appearance of care and cure, which in fact neither promotes quality or quantity of life. The complementary therapists' alliance with Cairns' dissent has a boundary work function in their contest for authority. Despite his dissent on this issue, Cairns carried with him the authority of those 'inside' science. For the complementary therapists, who for the most part are marginalized by the scientific community and denied authority, alliance with Cairns offered potentially increased authority by association. If a new point of contest could be established between the orthodox and complementary therapists then a boundary contest for authority could begin in relation to it. So, in the contingent forum there was explicit attention to Cairns'

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<sup>641</sup> It should be noted that the implicit rejection that Cairns experienced in relation to this publication did not extend to any greater degree of rejection or discredit. Subsequent to this publication Cairns continued to hold prestigious posts, receive funding and achieve publication in the most prestigious journals. It seems that, on account of his previous standing, this episode of dissent was largely overlooked by the scientific community.

publication based upon the interests, including boundary/authority interests, of various non-scientific groups.

Lerner goes on to make the observation that a cancer therapies debate, of the very nature that Cairns' had sought to incite, did in fact begin in 1986. At this time the journals became the forum for the conflict; that is, the cancer therapies debate was given explicit attention in the constitutive forum. However, as Lerner points out, it was not Cairns' article that precipitated this debate, but rather a very similar offering from two of his Harvard colleagues, entitled 'progress against cancer?'.<sup>642</sup>

Cairns' 1985 commentary on the war against cancer did not result in his discredit. Although he had made comments that were embarrassing to the medical profession, he had used statistical analysis to demonstrate that his was one valid reading of the situation. He had also made some rather strong political statements, but these were perhaps forgiven as frustration after a career working in what still seemed a desperate and under-funded fight against a poorly understood disease. On this issue Cairns was perhaps in an unarticulated position of disgrace. He had not been 'punished' as such in the Mertonian sense, but he had marginalized himself and the community had acknowledged that tacitly by ignoring his contribution. In 1985 Cairns had not made any explicit new claims, he had simply offered an interpretation based on existing data. He had however, through his interpretation, challenged the legitimacy of the mainstream approach from within the ranks of its authority. Not until 1988 would Cairns offer dissent that coupled interpretation with new data and unorthodox knowledge claims.

## 2. Directed mutation: Resurrecting Lamarck?

Cairns' first report of directed mutation appeared in 1988 (see Chapter 2).<sup>643</sup> In that first paper Cairns, and his Harvard team, made three assertions:

- i) That a directed mutational process appeared to act in populations of starved bacterial cells.

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<sup>642</sup> Bailer, J. & Smith, E. (1986) Progress against cancer? *New England Journal of Medicine*, 314: 1226-1232.

<sup>643</sup> Cairns, J., Overbaugh, J. & Miller, S. (1988)

- ii) That directed mutation in bacteria implied the activity of a Lamarckian type evolutionary process.
- iii) That directed mutation in bacteria might imply the existence of non-Darwinian evolution in other organisms.

Of these three assertions the second and third are the most controversial. The team's claim to have observed directed mutation in bacteria is not itself problematic. In isolation the observation represents an anomaly, and anomalies are neither rare nor necessarily controversial in science. The controversial element arises from the authors' discussion of the implications of this anomaly. In framing those implications they committed two acts of unorthodoxy, and incited controversy at two levels. Firstly, the use of Lamarckian reference implied the teams' engagement in the longer running conflict between Lamarckians and Darwinians, and appeared as an attempt to resurrect the contentious material of Lamarckian theory. Second, this team of molecular biologists were offering statements about possible macro-evolutionary processes; an act that fell outside the disciplinary authority that they possessed as molecular biologists. Whereas Cairns' 1985 statements about cancer were unpopular with the mainstream, his 1988 proposal of a Lamarckian anomaly placed him firmly in association with unorthodoxy. The old map identities of Lamarckism and Darwinism (see Chapter 3) firmly recorded the delineation of 'inside' from 'outside' in relation to the theories. Cairns' Lamarckism placed this contribution in the area that the old map recorded as 'outside'. He was reminded of this in the subtle threat implied by Lenski and Ayala in 1989 when they stated that the invocation of Lamarckism 'may perpetuate mistaken beliefs that are still widely held *outside* scientific circles'<sup>644</sup> (my emphasis).

In the wake of the 1988 Harvard paper<sup>645</sup> critics answered Cairns' heterodoxy, and for the most part sought to reduce the impact of his dissent either by offering a non-contentious explanation for directed mutation or by demonstrating that some element of the experiments rendered them invalid. Not only was the

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<sup>644</sup> Lenski, R., Slatkin, M. & Ayala, F. (1989) p.2778

<sup>645</sup> Cairns, J., Overbaugh, J. & Miller, S. (1988)

response to his publication explicit, but also it was conducted in the key journals, and was engaged with by key authors in the fields of evolutionary biology and molecular biology. In Collins' and Pinch's terms, attempted rejection of directed mutation was explicit in the constitutive forum.<sup>646</sup> For example, the 1988 paper was accompanied by a review from Nobel Prize winner and fellow member of the 'phage group' Franklin Stahl.<sup>647</sup> Also, it appeared in the journal *Nature*, indicating the intention to make the negotiation of directed mutation a constitutive rather than contingent project. It is perhaps a legacy of Cairns' eminent status that the negotiation of this claim was made so explicit. Whereas unorthodoxy from a little known or regarded individual or group might be easily rejected implicitly, a contribution from a prestigious institute, and esteemed academic is more difficult to overlook. An analogous case is offered by Jacques Benveniste's reports of water memory in the 1980s. Those reports also appeared in *Nature* and were also dealt with explicitly by the scientific community. Jacques Benveniste was a regarded scientist and his research was conducted at the prestigious INSERM 200 laboratory of the University of Paris.

Cairns' advocacy of these two contentious issues during the 1980s had some visible outcomes in terms of the kinds of 'punishments' that Merton predicts for those who offend against the scientific 'norms' or promote unorthodoxy.<sup>648</sup> While both his chemotherapy article and his directed mutation research achieved publication, there are some aspects of their style of publication that betray the different treatment of these more contentious contributions. For example, Cairns' 1985 paper was published in the journal *Scientific American*. In their study of legitimation and rejection Collins and Pinch identify that journal as part of the 'contingent' forum for debate.<sup>649</sup> In their analysis, debate in the 'contingent' forum is peripheral to debate in the 'constitutive' forum. In the 'constitutive' forum, which is comprised of the major journals, professional conferences and reviewed books, the actual articulation and discourse of debate occurs. The debate in the 'contingent'

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<sup>646</sup> Collins, H. M. & Pinch, T. J. (1979)

<sup>647</sup> Stahl, F. (1988)

<sup>648</sup> See Merton, R. (1942)

<sup>649</sup> Collins, H. M. & Pinch, T. J. (1979)

forum is supplementary to that discourse and includes personal communications, popular writing and publications in the less specialist or regarded journals. So in terms of Collins' and Pinch's framework, Cairns' 1985 article, although achieving publication, does so only as part of the contingent discourse, despite Cairns' usual acceptance as a contributor to the constitutive forum. In Merton's terms, this apparent demotion to the contingent forum might be a kind of 'punishment' for unorthodoxy.<sup>650</sup>

To understand the circumstances of publication pertaining to Cairns 1988<sup>651</sup> it is useful to consider Collins' and Pinch's discussion of 'the dilution of orthodox publication'.<sup>652</sup> These authors suggest that unorthodox material sometimes achieves publication in the mainstream journals as part of the 'tactics for rejection'. In those cases the material appears in a journal that is recognised as part of the constitutive forum, but is featured alongside an article that suggests caution or caveats to the reader. Thus, although legitimacy appears to have been achieved, it is diluted by a qualification that signposts the unorthodoxy. In some cases this combination of publication and caveats is formulated to the degree that the publication becomes a kind of tokenism.<sup>653</sup> In other cases, the qualifying article suggests that the unorthodox material should be approached in the spirit of 'discussion' or 'hypothesis'.<sup>654</sup> The implication is that its content is invalid, but that if viewed as a debating exercise it might be made useful to the mainstream community. *Nature* editor John Maddox has explained journal publishers' motivation for including these kinds of qualified publication, stating that: 'Journals are these days painfully aware of the accusation that their collective influence is to inhibit innovation.'<sup>655</sup> So, journals include this kind of material to achieve an appearance of openness. Maddox hints at the boundary function of these publications, saying: '...even when there is

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<sup>650</sup> An alternate reading of this situation would be that Cairns perhaps chose *Scientific American* as the forum for this paper, seeking to reach its wider and more diverse readership. In that case, this would provide a further example of his self-conscious role as an advocate, and would contribute another element to his 'loudness' (see 4.2.5)

<sup>651</sup> Cairns, J., Overbaugh, J. & Miller, S. (1988)

<sup>652</sup> Collins, H. M. & Pinch, T. J. (1979)

<sup>653</sup> Collins, H. M. & Pinch, T. J. (1979)

<sup>654</sup> Gregory, J. (1998)

<sup>655</sup> Maddox, J. (1988b) Waves caused by extreme dilution. *Nature*, 335: 760-763. p. 761

little chance that the heterodox will become orthodox; people may find it instructive to know what is happening *on the fringes* of their interest'<sup>656</sup> (my emphasis). These articles therefore are ostensibly published to promote openness, but in fact appear as a reminder of the boundary between science and non-science.

Cairns' directed mutation paper appeared in the journal *Nature*, which seems to confer acceptance in the constitutive forum. However, the paper was featured alongside a review article that provided suggestions of how the research might be approached by readers. The review article was provided by respected molecular biologist Franklin Stahl and it stated that Cairns' article should be read in the spirit of enquiry and for its novelty and potential interest.<sup>657</sup> He suggested that readers might 'find it a good exercise to try to build more specified schemes.'<sup>658</sup> Essentially, the presentation of unorthodox material has a kind of training function for mainstream scientists; perhaps helping them build personal articulations of the boundaries of science. Stahl entitled the paper '*A Unicorn in the Garden*' and explained later<sup>659</sup> that he had meant that if someone told you there was 'a unicorn in the garden' (i.e. something unbelievable and strange) that you would still go and have a look. Stahl is suggesting that the first report of directed mutation is like a report of a unicorn in the garden, and that as such it should at least be taken a look at. He signals the need for boundary work to resolve the challenge offered by directed mutation, saying that, a decision regarding the validity of the phenomenon awaits '...experimental support for some explanation that makes use of familiar processes...'<sup>660</sup> By 'familiar' Stahl means orthodox, and is asking the community to seek an explanation that negates the challenge of the anomaly. He even says that: 'Top marks will go to those who can build their model solely from familiar elements.'<sup>661</sup> Making reference to a forthcoming paper from Barry Hall, Stahl adds:

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<sup>656</sup> Maddox, J. (1988b) p.761

<sup>657</sup> Stahl, F. (1988)

<sup>658</sup> Stahl, F. (1988) p.112

<sup>659</sup> Stahl, F. (1990); Stahl, F. (1992)

<sup>660</sup> Stahl, F. (1988) p.113

<sup>661</sup> Stahl, F. (1988) p.112

‘You should be warned, however, that more difficult challenges are just over the horizon.’<sup>662</sup>

An analogous example of this kind of qualified or ‘diluted’ publication appears in the journal *Nature* just two volumes prior to the publication of *The Origin of Mutants*.<sup>663</sup> In that issue Jacques Benveniste’s controversial article<sup>664</sup> describing ‘water’s memory’ appeared alongside a qualifying/caveat article from the journal editor John Maddox.<sup>665</sup> Maddox suggested that Benveniste’s work be read for interest rather than information, stating that: ‘There are good and particular reasons why prudent people should, for the time being, suspend judgement.’<sup>666</sup> Maddox then proceeded to engage personally in the conflict that the initial publication generated (See Chapter 5.6 for a fuller account of this episode). He led a delegation to Benveniste’s lab to observe their assays and then reported in the next volume of *Nature* that the results they had obtained appeared flawed.<sup>667</sup> When Cairns’ first paper on directed mutation appeared Maddox was still addressing the water memory debate in the commentary sections of the publication.<sup>668</sup> The caveat article provided alongside *The Origin of Mutants* came from Franklin Stahl, perhaps because Maddox was too busy dealing with the aftermath of the previous ‘publication with qualification’ that he admitted to the journal just two volumes earlier. It is worth considering that Maddox was perhaps a particular enthusiast of the ‘diluted publication’ tactic that Collins and Pinch<sup>669</sup> identify as a form of implicit rejection in the constitutive forum.

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<sup>662</sup> Stahl, F. (1988) p. 113

<sup>663</sup> Cairns, J., Overbaugh, J & Miller, S. (1988)

<sup>664</sup> Davenas, E., Benveniste, J. *et al* (1988)

<sup>665</sup> Maddox, J. (1988a)

<sup>666</sup> Maddox, J. (1988a) p. 787

<sup>667</sup> Maddox, J., Randi, J. & Stewart, W. (1988) ‘High dilution’ experiments a delusion. *Nature*, 334: 287 With reply from Benveniste.

<sup>668</sup> Maddox, J. (1988b)

<sup>669</sup> Collins, H. & Pinch, T. (1979) p.258

#### 4.2.4 Boundary work in the directed mutation debate: advocates and adversaries

##### The critics' tactics

It is useful to consider the critics' response to directed mutation in terms of the categories that Collins and Pinch identify in their discussion of the rejection of rival knowledge claims.<sup>670</sup> To make sense of conflict activity, Collins and Pinch think about individual actions as 'tactics' purposed to achieve the defence of orthodoxy.

Collins and Pinch suggest that one key tactic adopted by the critics of a new knowledge claim is to formulate an *a priori* argument, or a negating precedent, against that new claim. Collins and Pinch call this 'using the symbolic and technical hardware of philosophy'.<sup>671</sup> This tactic is based on the implication that nothing can be true that is in direct conflict with what has already been established. That tactic is clearly illustrated in the directed mutation conflict. For example, in the very first critiques of directed mutation, and persistently thereafter, Luria and Delbruck's fluctuation test<sup>672</sup> is invoked as precedent against the directed mutation observations.<sup>673</sup> That experiment is cited as pre-existing proof that bacterial mutation is random with respect to selective advantage. The citation is used to imply that dissent on the issue of directed mutation has previously been arrested, that the new publication is in effect 'behind the times' in its assault on orthodoxy. For the critics, invocation of this precedent is purposed to end negotiation before it has started.

Cairns foresaw this tactic (see below) and so dedicated space even in the first Harvard directed mutation publication to asserting the difference between their assays and the fluctuation test. In spite of that pre-emptive defence, and many

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<sup>670</sup> Collins, H. & Pinch, T. (1979) Only those tactics that are evident in the directed mutation case study are discussed here. Collins and Pinch identify several other categories of tactics, ranging from the most explicit such as the 'denial of orthodox publication' to the more subtle such as 'accusations of triviality' and 'ad hominem arguments'.

<sup>671</sup> Collins, H. & Pinch, T. (1979) p.242

<sup>672</sup> Luria, S. & Delbruck, M. (1943)

<sup>673</sup> Franklin Stahl begins his introduction to the first Harvard publication on directed mutation with a summary of Luria and Delbruck's conclusions and says that '...John Cairns, Julie Overbaugh and Stephan Miller challenge these credentials.' [Stahl, F. (1988) p.112] See also [Stewart, F. Gordon, D. & Levin, B. (1990); Lenski, R. & Mittler, J. (1993a)]



subsequent explanations of the key differences between the experimental systems, the precedent of Luria and Delbruck was still being invoked many years later.<sup>674</sup>

Collins and Pinch suggest that another key tactic of critics is to attack the methodological precepts upon which new knowledge claims are founded. Again in the case of directed mutation we see this approach in action. In the first responses to the Harvard report the methodology of the directed mutation assays is scrutinised and numerous potential oversights are highlighted and implied.<sup>675</sup> Answering these, and the later methodological queries and complaints became one of the major tasks of the directed mutation advocates. Cairns, and the other advocates, focussed significant attention on these issues in the early years of the debate, often directly addressing the methodological ‘problems’ that the critics had highlighted.<sup>676</sup>

A further tactic employed in the directed mutation debate, which I suggest might be added to the classification that Collins and Pinch<sup>677</sup> offer, is the ‘invocation of unfavourable historical precedent’ for the contested knowledge claim. This is accomplished by highlighting previous unsuccessful attempts to establish the same (or a similar) contested knowledge claim. This amounts to the assertion that a new claim cannot be legitimate because it (or something similar to it) has been rejected in the past. In the case of directed mutation, some critics seized upon other examples of ‘Lamarckian’ work to show that Lamarckian claims in general are not legitimate. This tactic was used in the contingent forum, where appeals to this kind of self-fulfilling historical evidence are not uncommon. For example, in 1995 Daniel Dennett wrote about ‘three losers: Lamarck, Theilard and directed mutation’.<sup>678</sup> In his construction directed mutation effectively follows in the footsteps of these other rejected claims.

### The advocates’ tactics

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<sup>674</sup> For example see Rosenberg, S. *et al* (1994); Foster, P. (1994)

<sup>675</sup> See for examples: Charlesworth, D & Charlesworth, B. & Bull, J.(1988) ; Holliday, R. & Rosenberger, R. (1988); Van Valen, L. (1988); Danchin, A. (1988).

<sup>676</sup> See for examples Cairns, J., Overbaugh, J. & Miller, S. (1988); Cairns, J. (1988); Cairns, J. & Foster, P. (1991); Foster, P. (1992); Foster, P & Cairns, J (1992).

<sup>677</sup> Collins, H. & Pinch, T. (1979)

<sup>678</sup> Dennett, D. (1995)

The 1988 Harvard paper had framed a challenge to the authority and monopoly of evolutionary biologists. Cairns' team had asserted a bid for the expansion of the authority of molecular biology into territory usually delineated as the domain of evolutionary biologists. The advocates of directed mutation, John Cairns in particular, carried out numerous activities in service to that bid for expansion. Those activities constitute the advocates' boundary work for expansion. The journal articles that appear early on in the debate illustrate that the advocates of directed mutation were self-aware of the need to strengthen their position in relation to the authority of the evolutionary biologists. Likewise, their adversaries were aware that the directed mutation publications represented an attempt by an adjacent disciplinary group/territory to increase authority into the territory they had claimed for themselves in previous contests.

Cairns carried out several acts of boundary work to promote the advocates' success in this contest. He used certain tactics to assert the new knowledge claim, to sustain its assault and to strengthen its impact. There are three key examples of that activity:

i) I describe above the critics' tactic of invoking negative precedent against the directed mutation research in the form of references to Luria and Delbruck's fluctuation test. Cairns' response to that critique was pre-emptive. He anticipated that Luria and Delbruck (1943) would be invoked, and dedicated significant space in '*The Origin of Mutants*' to describing the important distinction between the Harvard research and the assay that Luria and Delbruck had carried out. He asserts that Luria and Delbruck's experiments bear no relation to the directed mutation assays due to important methodological differences (see Chapter 2). From the outset Cairns and his team considered potential criticisms, and tailored their presentation to increase the vigour of their challenge. This pre-emption of, and attention to, forthcoming criticisms of directed mutation constitutes one of the principal tasks that Cairns' undertook as an advocate.

ii) Although it may seem counter intuitive, Cairns' adoption of Lamarckian rhetoric (see details in Chapter 2) might be viewed in part as another tactic of boundary work for expansion. Collins and Pinch argue that a key tactic of critics is to defame new dissenting knowledge claims by equating them with existing 'unscientific' beliefs or unorthodoxy.<sup>679</sup> The nature of directed mutation predisposes it to association with Lamarckianism. The theory supposes interaction between the environment and DNA and, in its strong version, even an element of cell 'choice'. Critics and commentators immediately addressed these links.<sup>680</sup> However, Cairns and his team did not attempt to avoid that association, but rather made reference to it themselves in the 1988 paper. In that light, the group's Lamarckism is another pre-emptive act designed to strengthen their challenge by making it immune to 'unexpected' criticism.

iii) I describe above the critics' tactic of 'attacking the methodological precepts' of unorthodox knowledge claims. Collins and Pinch identify this as a key form of explicit rejection in the constitutive forum.<sup>681</sup> This kind of critical attack, and the defence mounted in relation to it, form a familiar part of the discourse generally associated with controversy. Again, Cairns' approach to this criticism begins pre-emptively. Much of the discussion in the initial 1988 paper is dedicated to explanation and clarification of methodology. I have mentioned above that Cairns' team devoted attention to distinguishing their methodology from Luria and Delbruck's fluctuation test as a means by which to deflect that comparison. Although methodological criticisms of the Harvard research still formed a major part of the negative response to the initial publication, many points had been clarified and pre-empted prior to the first publication.

Cairns, and the other directed mutation advocates, demonstrated an awareness of approaches that might strengthen their challenge. Their early journal

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<sup>679</sup> Collins, H. & Pinch, T. (1979)

<sup>680</sup> For example, Lenski, R., Slatkin, M. & Ayala, F. (1989) said that Cairns' Lamarckism was '...potentially harmful in that it may seem to give credence to prescientific claims that have been thoroughly disproved.' p.2777

<sup>681</sup> Collins, H. & Pinch, T. (1979)

articles demonstrate defences against the key tactics of their critics; they assert their claim, answer queries, and defend their research methodology. These are standard activities for a dissenting group attempting to gain credibility for a controversial knowledge claim. What is interesting in this case is that Cairns' defence began pre-emptively in each case. His pre-emption of criticisms implies that he had dedicated significant care to his role as an advocate, and considered his agency in the contest. Cairns did not wait for a defence of orthodoxy to be mounted against directed mutation, but rather guessed what its quality and content would be and defended the theory in advance. Thus, the 1988 paper that initiated the debate might be viewed as a crafted tool of the molecular biologists' boundary assault, the majority of the text being given over to pre-emptive defence against 'a priori argument', 'association with unscientific belief' and 'attacks on methodological precepts'.

### Why be a Lamarckian?

The defences of directed mutation that Cairns and the other advocates present are tools for the perpetuation of their knowledge claim. That perpetuation increases the likelihood of their success. Some of these activities are overt, and are clearly directed to prolong debate. In addition to these acts, Cairns' advocacy includes what might be considered as more cryptic boundary work activities for authority expansion. A key example is the invocation of Lamarckism that marks Cairns' advocacy from the outset.<sup>682</sup>

To evaluate the possible boundary work function of Cairns' Lamarckism, we must consider the value of Lamarckian association as a rhetorical device. In this section I argue that Lamarckian association can be seen as i) a rhetorical device signifying dissent, ii) a device for cohering a group of dissenters on the margins of orthodoxy and iii) a tool of advocacy relying upon sensationalism.

Chapter 3 describes the old map identity of Lamarckism that emerged during the twentieth century. In that chapter I show that the old map summarises failure and

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<sup>682</sup> See discussion in Chapter 2 of the Lamarckian rhetoric in Cairns, J., Overbaugh, J. & Miller, S. (1988)

records the stigmatisation of Lamarckian association. I suggest that, by the late twentieth century, the map had gained a degree of persistence and cultural acknowledgement that meant it was consulted during almost any episode of dissent in evolutionary biology. That is to say, by the late twentieth century the presence of the old map meant that Lamarckism was irreversibly stigmatised and not a valid contestant for authority in science. To be a Lamarckian became inevitably controversial owing to that legacy. To acknowledge Lamarckian content in one's work, as Cairns' original team did, was to brand oneself controversial or dissenting. Historian Peter Bowler has considered the role of what he calls the 'label' Lamarckism,<sup>683</sup> suggesting that historically the label or brand has been used to signify dissent.

So, we might consider Cairns' early acknowledgement of the Lamarckian links of directed mutation as a tactic for branding his work as dissent. Rather than proposing a microbial genetics anomaly, Cairns launched a more profound challenge, the impact of which reached other disciplines. To be visible to the broad community, the material of the dissent had to first attract attention. What better way to attract the necessary attention for that challenge than to invoke the old map of Lamarckism? By encouraging the community to consult the old map Cairns was encouraging a resurrection of the Lamarckism versus Darwinism debate, and in so doing he immediately increased the life expectancy and scale of his dissent. The effect of Cairns' use of the 'Lamarckian' label was acknowledged by biologist Bernard Davis in 1989, saying that: '...even if Cairns' tilt towards Lamarckism should have to be rejected, it has served a very useful purpose, stimulating us to reconsider a long series of stubborn facts that have suffered neglect because they seemed hard to reconcile with Neo-Darwinian doctrine.'<sup>684</sup> Davis acknowledged that Cairns' Lamarckism extended the challenge of directed mutation beyond the field of

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<sup>683</sup> Bowler argues that the late nineteenth century advocates of what they termed Neo-Lamarckism had not re-invented Lamarckism as such, but rather had revived accounts of Lamarckism from Haeckel and Spencer. Bowler suggests that they used the 'label' Lamarckism to highlight their dissent against Darwinism. [Bowler, P. (2003) Wellcome Trust symposium on transformism, evolutionism and creationism. The Wellcome Trust, London 06/12/03; Bowler, P. (2003) Personal communication.]

<sup>684</sup> Davis, B. (1989)

molecular biology, attracting the attention of other specialists and provoking a broader debate than the directed mutation anomaly alone would precipitate.

Invoking the legacy of Lamarckian resurrections also served to locate the directed mutation anomaly as part of a larger and more ongoing conflict. This meant that rather than the directed mutation researchers launching a contest in isolation, they had instead framed their dissent as the most recent incarnation of an ongoing challenge to orthodoxy. While the critics of directed mutation raised precedent against the observations, the invocation of Lamarckism allowed the advocates to highlight precedent for similar dissent. Even though that precedent raised the spectre of defamation by association (discussed above), it rendered this new challenge part of a bigger and longer-term challenge to the orthodoxy of evolutionary biology. By making the Lamarckian association the advocates affected a kind of passive recruitment of those authors that had previously made similar challenges.

Finally, the stigma attached to Lamarckism (recorded on the old map) also meant that any reference to the theory would be bound to provoke response; in the late twentieth century Lamarckian claims were sensational. Although Cairns' Lamarckism attracted negative attention in the constitutive forum, it still increased the visibility of the debate. This tactic operates on the premise that 'there is no such thing as bad publicity'.

#### 4.2.5 John Cairns versus Barry Hall: 'loud' versus 'quiet' advocacy.

*The Origin of Mutants* certainly attracted attention, and commentaries on the work emerged immediately and abundantly from a wide variety of sources, both professional and popular (see Chapter 2). That outcome might appear inevitable; the publication was certainly controversial on several levels. The response that followed implied that directed mutation was intrinsically controversial, and that debate was the unavoidable outcome of the announcement of that bacterial anomaly.

However, an interesting alternative interpretation becomes apparent when the work of molecular biologist Barry Hall is considered. Hall had worked on bacterial nutrition and mutation since the early 1980s, and had published his findings in the

journal *Evolutionary Biology*.<sup>685</sup> Hall had in fact studied the exact same phenomenon that would later be labelled ‘directed mutation’ when *The Origin of Mutants*<sup>686</sup> was published. However, he had not invoked Lamarckism in his treatment, nor had he suggested that the mutational process occurred elsewhere than in the specific system he had studied (conversely, Cairns suggested it might occur in ‘cells’<sup>687</sup>). Hall’s report was very much restricted to the statement of a bacterial anomaly. He avoided sensationalism in his presentation. Even after the publication of *The Origin of Mutants*, and the commencement of the flurry of debate that John Cairns calls the ‘brouhaha’<sup>688</sup>, still Hall avoided sensationalism in relation to his observations. In his 1990 paper on the bacterial mutational phenomenon he termed the process ‘spontaneous point mutations that occur more often when advantageous than when neutral’<sup>689</sup>; a much less catchy and inflammatory tag than ‘directed mutation’. In that same paper Hall also used the term ‘Cairnsian mutation’ to describe the adaptive mutational process, distancing himself as an advocate, and shifting attention back towards Cairns.

Hall’s work did not precipitate a controversy of the kind that the ‘*The Origin of Mutants*’ stimulated. Hall reported the phenomenon as a bacterial anomaly, and as such it seemed almost unproblematic. It certainly did not seem to represent an authority challenge to evolutionary biology of the kind that the Harvard team later asserted. Although Hall’s findings were very similar to those that the Harvard team later published, he had constructed his report without making any explicit or articulated boundary assault; his work differed from Cairns’ in terms of tone and presentation. So, directed mutation in bacteria is not necessarily controversial *per se*. Neither are the echoes of Lamarckism that the Harvard team emphasised necessarily intrinsic to the discussion of this anomaly. Directed mutation is not necessarily an issue that need bear implications for evolutionary biology, provided that those implications are not asserted hand in hand with observations of the system. Hall referenced his more moderate approach himself saying: ‘I accepted Cairns’

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<sup>685</sup> Hall, B. (1982b)

<sup>686</sup> Cairns, J., Overbaugh, J. & Miller, S. (1988)

<sup>687</sup> Cairns, J., Overbaugh, J. & Miller, S. (1988)

<sup>688</sup> Personal communication 10/2003

<sup>689</sup> Hall, B. (1990a)

argument about the limits of the Luria-Delbruck work, but I was firmly rooted in conventional dogma'.<sup>690</sup> Although Hall brought the issue of directed mutation to the evolutionary biological community by publishing in the journal of evolutionary biology, he did not demand the communities' explicit engagement with the anomaly by vocalising the challenge that it might present. Hall presented the material of the directed mutation anomaly to the community of evolutionary biologists, but he did not frame his report as a challenge to the authority of that disciplinary group.

Cairns' and Hall's publications also rarely shared the same publication arena. Barry Hall chiefly presented his research in the journal *Genetics*, while Cairns' contributions most often appeared in *Nature* and *Science*. Interestingly, despite the fact that Barry Hall was one of the most prolific authors of the debate he did not ever co-author with John Cairns. In fact, Hall's contributions on directed mutation are almost exclusively single author pieces. Whereas Cairns was interested in recruitment and the formation of a network of directed mutation advocates, Hall appeared to remain abstracted and isolated from the community of those researchers. This reflects the motivations underlying each of their engagements. Hall was apparently interested only in reporting the details of the bacterial anomaly as a comparatively uncontentious phenomenon relevant to a small community. Cairns meanwhile had framed the anomaly as the foundation of an authority dispute, and as such was obliged to adopt another approach as part of the boundary work for expansion that he had set in motion.

Hall's different quality of approach from Cairns' has vastly influenced the way that these authors' roles in the debate are evaluated. The majority of commentators cite Cairns as the 'discoverer' of directed mutation, and almost all popular and even professional commentaries refer back to '*The Origin of Mutants*' as the primary publication on the phenomenon. Accounts in print and online record the beginning of the directed mutation debate with that publication in 1988. This appraisal is perhaps a fair reflection of the history of the debate. Hall did not in fact instigate the second, and most vigorous, element of the conflict (that concerned with Lamarckism versus Darwinism). Neither did he instigate the element of the debate

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<sup>690</sup> Reported from personal communication in Goodman, B. (1992) p.29



attached to the relative authorities of the molecular biological and evolutionary biological communities. Barry Hall may have ‘discovered’ directed mutation, and have earned priority in that respect, but John Cairns and his Harvard team instigated the directed mutation debate.

When the Harvard directed mutation observations were reported in 1988, Hall made no complaint concerning his priority in this field. However, interestingly, a priority claim was received by *Nature* (and the president of Harvard University) from Australian immunologist Edward Steele. In fact, Steele went as far as to accuse Cairns’ team of plagiarism and a separate dispute ensued in the pages of *Nature*.<sup>691</sup> That conflict angered *Nature* editor John Maddox, who said: ‘A dispute over the attribution of priority for a neo-Lamarckian mechanism is premature and unseemly, to say the least, and may help make science seem ridiculous.’<sup>692</sup> Steele had published his theory of acquired immunity in the early 1980s<sup>693</sup>, and had invoked reverse transcriptase in the process by which parental acquired immunity could be passed to offspring. Steele’s theory of acquired immunity bore scrutiny, and John Maddox admitted that it implied that ‘Lamarckian inheritance is alive and well, if only in exceptional circumstances’.<sup>694</sup> Steele’s plagiarism accusations stemmed from Cairns’ invocation of reverse transcriptase as an agent of directed mutation.<sup>695</sup> Cairns had not referenced Steele in relation to that hypothesis.

Maddox takes up the accusations in an editorial address, stressing that they are inappropriate, and reprimanding Steele for poor conduct. He concludes directed evolution is too far from proof to even consider who might take credit for it. He says: ‘...in a field (if it is one) that has not yet ever got a Copernicus, Steele is already talking like a Galileo.’<sup>696</sup> Maddox reassures the readership, and presumably Steele, that had the Harvard data been received as anything more than a point of interest and debate, then reviewers would have insisted that Steele receive a citation.

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<sup>691</sup> Maddox, J. (1989a) Lamarck, Dr Steele and plagiarism. *Nature*, 337: 101-102.

<sup>692</sup> Maddox, J. (1989a) p.101

<sup>693</sup> Gorczynski, R. & Steele, E. (1980) Inheritance of acquired immunological tolerance to foreign histocompatibility antigens in mice. *Proceedings of the National Academy of Sciences USA*, 77(5): 2871-2875; Gorczynski, R. & Steele, E. (1981)

<sup>694</sup> Maddox, J. (1989a) p.101

<sup>695</sup> Cairns, J., Overbaugh, J. & Miller, S. (1988)

<sup>696</sup> Maddox, J. (1989a) p.101

The dispute was resolved eight months later when Steele and Cairns presented a joint letter to *Nature*.<sup>697</sup> In that the authors cited Steele's acquired immunity publications, as well as Cairns' Harvard publication, and attributed both as an application of Howard Temin's elucidation of the enzyme reverse transcriptase. The short letter concludes with the statement that: 'One of us (E. J. S), in a letter he distributed to many people, has claimed that the other (J.C.) was guilty of plagiarism. He now acknowledges that the allegation was unfounded and he withdraws it.'<sup>698</sup>

Hall and Steele's different approaches to priority in the field of directed evolution relate to their motivations for engagement in the debate. Steele has been an ardent Neo-Lamarckian, keenly framing his observations of acquired immunity as evidence in favour of Lamarckism. Most recently he set about suing the University of Wollongong, which he claimed dismissed him unfairly because they did not want to be associated with his Lamarckian sympathies. So Steele was interested in the broader aspect of the directed mutation debate, the Darwinian versus Lamarckian conflict. He was also interested in the authority contest with evolutionary biologists that the Harvard team had launched. In that light it made sense for him to engage with the community that were involved in that contest, and to desire priority in that debate. By contrast Hall was not engaged with that level of the dissent, and so for him the notion of priority was more in line with Maddox's interpretation – 'premature' and 'unseemly'

I suggest that a useful way to characterise the different styles of Cairns and Hall is to characterise them as 'loud' and 'quiet' advocates respectively. This language effectively illustrates their different styles. I would like to suggest these might make useful general categories for considering advocates' styles. In general, 'loud' advocates are prone to sensationalism, dedicated to the acceptance of their claims, keen to extrapolate from observation to theory and eager to answer critics. They are unapologetic and willing to attach their reputations to the success of their claims. They are not averse to framing their claims as authority challenges. Loud

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<sup>697</sup> Steele, E. & Cairns, J. (1989) Dispute resolved. *Nature*, 340: 336.

<sup>698</sup> Steele, E. & Cairns, J. (1989) p.336

advocates are recorded historically as the principal advocates in contests. Conversely, 'quiet' advocates are modest in the presentations, focussing on observations without extrapolation to implications. They avoid framing authority contests and history records their advocacy as secondary to that of the loud advocates.

In section 4.2.6 I demonstrate the potential broad applicability of the categories 'loud' and 'quiet' by exploring the analogous styles of advocacy in the Cold Fusion debate.

#### 4.2.6 An analogous case: Jones, Pons and Fleischmann's styles of advocacy in the Cold Fusion debate.

The Cold Fusion controversy provides an analogous example of different styles of advocacy. That debate featured two loud advocates, Stanley Pons and Martin Fleischmann, and one quiet advocate, Steven Jones. Pons and Fleischmann's advocacy shares many qualities with Cairns', while Jones' is similar to Hall's. The interaction of loud and quiet advocacy in the Cold Fusion debate had very similar outcomes to those I have identified in the directed mutation debate (see 4.2.5).

In accounts of the Cold Fusion debate Pons and Fleischmann are generally recorded as the originators of the anomalous findings, and as the principal advocates during the months of the contest. They revealed their findings in a sensational and unorthodox press conference, made huge claims about the implications of their research for energy production, and devoted their time to trying to ensure the success of their claims. While Cairns did not behave in such an unorthodox manner, his advocacy does share its character with Pons and Fleischmann's. All three used sensationalism as a tool for drawing attention to their initial claims; while Pons and Fleischmann called a press conference, Cairns did something equally dramatic. He claimed in print that observations in molecular biology had revealed a deficit in Neo-Darwinian theory, and even went as far as to invoke the spectre of Lamarck. The three advocates shared the approach of making unapologetic claims, with an emphasis on the possible huge implications of their findings. Pons and Fleischmann

stressed the potential for amazing energy producing technologies, Cairns stressed the fact that directed mutation might occur in all 'cells' and might completely change our view of organic evolution; they all extrapolated wildly from laboratory observations to speculative theories and implications. After the initial claims, Cairns dedicated efforts to repeating and reinforcing the observation of directed mutation. He answered critics in journals and through personal communication. He was engaged in the work of advocacy, striving for the acceptance of his team's claims. Similarly, Pons and Fleischmann took action to further their claim. They attended meetings, made bids for funding and acted as ambassadors for Cold Fusion.<sup>699</sup> All three also shared a willingness to frame their dissent as an interdisciplinary contest. Pons and Fleischmann made an unapologetic transgression into the territory of physics, Cairns made a similar assault on the authority of evolutionary biologists. They presented their research not just as a problem for one specialist group, but as a challenge to the structure of the specialisms and the authority they confer.

These three qualify as loud advocates on account of their shared dedication to their claims; in the face of severe criticism, and with reputation pitched on their success in the contest. As a result history records them as the principal figures in the debates, and marks their first contributions as the instigation of the contests. All three are responsible not just for a theoretical debate, but also for an interdisciplinary contest, and for a challenge to fundamental tenets of existing theory.

In the case of directed mutation I have demonstrated that this was not the only possible approach to the directed mutation observations, and not the only style of advocacy that could be selected. I have described how Barry Hall made the same observations, but chose to present his findings in a completely different style. Where Cairns' presentation was marked by sensationalism, Hall's was marked by modesty and temperance and was free from speculation on implications. Hall did not frame a disciplinary contest. I have characterised him above as a 'quiet' advocate in that contest.

In the case of Cold Fusion, Jones is the quiet advocate. Although Pons and Fleischmann are recalled as the progenitors of the theory, Jones had in fact been

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<sup>699</sup> See Collins and Pinch (1993) for details of their advocacy activities between April and July 1989

working on the phenomenon for longer than their team. Just as Hall had worked from the early 1980s to reveal directed mutation, so Jones had been investigating Cold Fusion through dozens of experiments long before Pons and Fleischmann exploded onto the scene.<sup>700</sup> Collins and Pinch have characterised Jones' approach as 'modest', and pointed out that his claims 'did not pose the same theoretical challenge.'<sup>701</sup> He did not frame a disciplinary contest, and he did not mention the energy producing technologies that Pons and Fleischmann were so keen to highlight. Jones presented his work as 'an interesting piece of physics',<sup>702</sup> playing down its significance just as Hall had done with directed mutation. The result of this 'quiet' approach has been noted by Collins and Pinch, who have stated that:

'...had it not been for Pons and Fleischmann, Steven Jones would probably have quietly established an interesting fact about the natural world...'<sup>703</sup>

It seems almost certain that Hall might have 'quietly' done the same thing without Cairns.

In the case of directed mutation the loud and quiet advocates combined their efforts very effectively and peacefully. Hall may not have contributed the forceful kind of advocacy that Cairns championed, but he did carry out dozens of the experimental replications and modifications that the critics demanded. He was contributing to the project of advocacy, and patiently answering critics through experiment. In the case of Cold Fusion the loud and quiet advocates did not form such a happy union. They became involved in a bitter fight for priority, and communication broke down between the two groups. The situation in the directed mutation debate was perhaps facilitated by the fact that Hall showed no interest in claiming priority for the observations, and was happy to call his phenomenon 'point

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<sup>700</sup> Collins & Pinch (1993)

<sup>701</sup> Collins & Pinch (1993) p.65

<sup>702</sup> McAllister, J. (1992) p.7

<sup>703</sup> Collins & Pinch (1993) p.65

mutations that occur more often when advantageous than when not<sup>704</sup> and let Cairns have ‘directed mutation’.

The results of loud and quiet advocacy in each of these debates has been the same. The loud advocates are recognised as the key advocates, and their quiet counterparts contributions are less visible. Ironically, while the loud advocates have fought for their claims so ardently, it is clear that the quiet advocates might have achieved a degree of acceptance for the same claims had their modest approach not been interrupted. That fact demonstrates that loud advocacy itself can perpetuate conflict, which supports my identification of Cairns’ advocacy as one of the active forces of protraction in the directed mutation debate.

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<sup>704</sup> Hall, B. (1990)

## **Chapter 5 – A new forum for perpetuation: Scientific controversy on the Internet.**

In this chapter I examine the role of the Internet in the directed mutation debate. I argue that the structure and dynamics of the debate were influenced by the Internet from the mid-1990s onwards. Using empirical data, I describe how the debate was extended both in scale of participation and meaning as it was taken up in this forum. I show how new communities engaged the debate and translated the dissent to suit their own agendas, making negotiation more complex and closure more elusive. I suggest that negotiation in the Internet forum has been a key force determining the perpetuation of the conflict.

With reference to the directed mutation case study, I argue that the Internet represents a new, important, and poorly understood forum for scientific debate. I suggest that a better understanding of engagement and dynamics in this forum will be key to successful analysis of late twentieth century scientific conflicts. I describe the difficulties that existing methodologies for controversy analysis face when they are applied to Internet hosted debates. In particular, I consider how boundary theory is impaired in contexts where the analogies of cartography break down. I ask can you have discredit, or even closure in the Internet forum? I consider what elements of existing methodologies might be salvaged to create an analytical approach suited to the study of scientific controversy on the Internet.

Finally, I compare the Internet phase of the directed mutation debate with the Internet debate on Jacques Benveniste's water memory, suggesting that change of scale and meaning, perpetuation of negotiation, and lack of resolution are perhaps more general outcomes of debates being taken up online.

### **5.1 How the Internet changed the scale of the directed mutation debate: 'following the object'.**

This section illustrates the increase of scale of the directed mutation debate on the Internet between 2000 and 2005. Chapter 2 has described the nature and extent of

the directed mutation debate in paper publication. The aim in this section is to follow the debate into the Internet forum and consider its identity in that new context. Bruno Latour has encouraged sociologists to make analyses that ‘follow the object’.<sup>705</sup> That is, as Bart Simon has interpreted the methodology, to trace ‘...the movement and deployment of the signifier...by accounting for its appearance, nonappearance and transformation in specific contexts.’<sup>706</sup> The approach involves viewing the directed mutation debate as a ‘quasi-object whose ontological status is in a state of flux’.<sup>707</sup> The meaning or identity of directed mutation at any point is then the ‘product of the network of human and non-human agents that are associated with it’.<sup>708</sup> The material in 5.1 and 5.2 is purposed to provide a characterisation of directed mutation as it is transformed in the Internet forum, the aim being to ‘follow the quasi- object’ of the debate into that phase of its history.

### **‘Directed mutation’ on the Internet in 2000**

In October 2000 a Web search, using the search engine Google.co.uk, and the search term ‘directed mutation’, yielded 233 hits. Analysis of these materials allows them to be categorised as several distinct ‘types’ of contribution to the directed mutation debate. I have identified 13 categories, and the proportion of the hits represented in each of these is depicted in fig.7. These include:

**Weird science:** This category includes commentaries on directed mutation, or reproductions of paper publications on directed mutation, that have been placed online to serve the explicit agenda of enthusiasts of the ‘the unusual and unexplained’ and ‘weird science’. Groups or individuals have created these pages for purposes of entertainment, or to promote what they describe as ‘scepticism in science’. In Chapter 2.3 I describe the rise of a culture of enthusiasm for science

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<sup>705</sup> Latour, B. (1993) *We have never been modern*. Harvard University Press, Massachusetts; Latour, B. (1996) Do scientific objects have a history? *Pasteur and Whitehead in a bath of lactic acid. Common Knowledge*, 5: 76-91.

<sup>706</sup> Simon, B. (2002) p.19-20

<sup>707</sup> Simon, B. (2002) p.19

<sup>708</sup> Simon, B. (2002) p.19



controversies and anomalies. These pages are produced in service to that culture. The materials in this category range from the reproduction of published articles, to lists of links, to commentaries written by interested amateurs. The most prolific and detailed site in this category is 'science-frontiers'<sup>709</sup>, which offers reproduced articles, discussion, commentaries, links and references to print publications. It prioritises 'those observations and facts that challenge prevailing scientific paradigms'. The site is produced by an amateur enthusiast and includes several references to directed mutation dating back to the late 1980s. This site is important in that many other online amateur authors reference it as their source of information on directed mutation.

Discussion: This category includes references to directed mutation in chat rooms, message boards, and news groups etc, including contributions from professionals and amateurs. The sites included in this category host discussion that is not linked to any particular agenda, for example weird science, teaching or religion. Rather these sites include online debate between amateurs and professionals, and the host site does not display or promote any particular interpretation of the material. These are general science chat rooms, evolution debate forums, or news discussion groups.

Teaching: This category includes references to directed mutation in online teaching materials - the majority in online undergraduate lecture notes. The material often appears as PowerPoint presentations, slides or syllabi posted on departmental websites. This is an important category, since it indicates the degree to which students are being taught about this controversy, and also reveals the identity of directed mutation being presented in teaching materials. Interestingly, in the majority of these materials directed mutation is described to students to assert the importance of random mutation as a fundament of Neo-Darwinian theory. Directed mutation is often equated with Lamarckism, and the theory is generally characterised as either problematic or rejected. Several online student quizzes or exams as why

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<sup>709</sup> [www.science-frontiers.com](http://www.science-frontiers.com) Described in more detail in Chapter 2.3

directed mutation is controversial, or ask for the proofs from molecular biology that contradict Cairns' findings (i.e. Luria and Delbruck).

Science and Religion: The materials in this category appear on sites dedicated either to the promotion of a religion, or explicitly pitched as anti-religious, and are produced by individuals, organisations and associations. The majority deal with issues relating to either the broad science versus religion debate, or the more specific, creation versus evolution debate. Interestingly, for some, directed mutation appears to support the theory of 'design' and shows intelligence and progress in evolutionary change. For others directed mutation shows that orthodox evolutionary theory is flawed and so open to challenge from creationists. Some of these sites offer reference lists or article reprints. This category also includes material linking the ascetic starvation practices of some religious groups to directed mutation. In their interpretation, the theory of directed mutation appears to offer a scientific endorsement of their practice of withholding nutrition in an attempt to achieve an altered spirituality.

Amateur online: This category includes online publications from amateur enthusiasts. The sites do not betray any overall religious agenda, and the bizarre and unexplained are not prioritised over other science news. These sites are often produced by a single individual posting their views on contemporary science issues online, or by groups that publish online magazines promoting amateur comment on science. The materials range from reproduced print publications, to commentaries, to annotated reading lists, to links lists.

Academic online: This category includes contributions from any author with affiliation to an academic institution. These are professional commentaries on directed mutation. The materials in this category do not serve a particular agenda, and are not explicitly purposed as teaching materials.

Journal articles online: This category includes any articles that have been reproduced online from original print publications. The article reproductions in this category appear on sites with no specific agenda, and offer no interpretation of the articles, or commentaries on them. In many cases these are publishing companies' websites offering articles or abstracts free of charge as part of 'open access' schemes. Many of the journal publications on directed mutation from the period 1988 – 1995 are available in this form. This category also includes the reproduced full text papers that are available through subscription, or for one off download fees.

Core-set online: This category includes online materials, concerning directed mutation, produced by members of the original core-set (identified in Chapter 2). It does not include reprints of their paper publications, only new material. The principal member of the core-set whose contributions appear in this category is Richard Lenski, who has several pages describing his work and providing full bibliographic information. Patricia Foster also presents directed mutation work on a personal website.

Interested academics: This category includes hits recording an individual academic simply citing directed mutation as a research interest.

Bibliographies: This category includes the references to directed mutation that appear in bibliographies or lists. The materials in this category do not display any interpretation of directed mutation, or discussion of the content of the citations. They are often library databases, general science databases, or journal content archives.

Citations in other materials: This category includes references to directed mutation which appear as asides in other projects. For example, Cairns' biographical material that cites directed mutation as one of his research areas, and some online encyclopaedias or science dictionaries that offer a definition of directed or adaptive mutation.

Genetic Algorithms: Genetic algorithms are a tool of computer programme engineering. Along with evolutionary algorithms they form a class of strategies called ‘stochastic optimisation principles’. Essentially, these algorithms function by applying rules from biological change processes to the development of programmes in artificial intelligence and computer network design. They are intended to allow programmes to ‘adapt’ to their function in the same way as organisms adapt to environments. The methodology was first introduced during the 1970s<sup>710</sup>, and the key principles that the strategy mimicked were crossover, mutation and selection. More recently the notion of directed mutation has been used as an algorithm. This is interesting in that it shows the uptake of the concept of directed mutation into another disciplinary area. The computer scientists that employ the directed mutation algorithm are not interested in the legitimacy of the scientific claims surrounding directed mutation, nor are they engaged in the negotiation of the phenomenon, they just use the notion as a conceptual tool.

Site-directed mutation: 70 of the 233 hits referred to site-directed mutation. These references are not relevant to the directed mutation debate, since they relate to an experimental procedure, called site-directed mutation, which allows manipulation of specific DNA loci for research purposes. Therefore, these references have been excluded from this sample.

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<sup>710</sup> Holland, J. (1975)

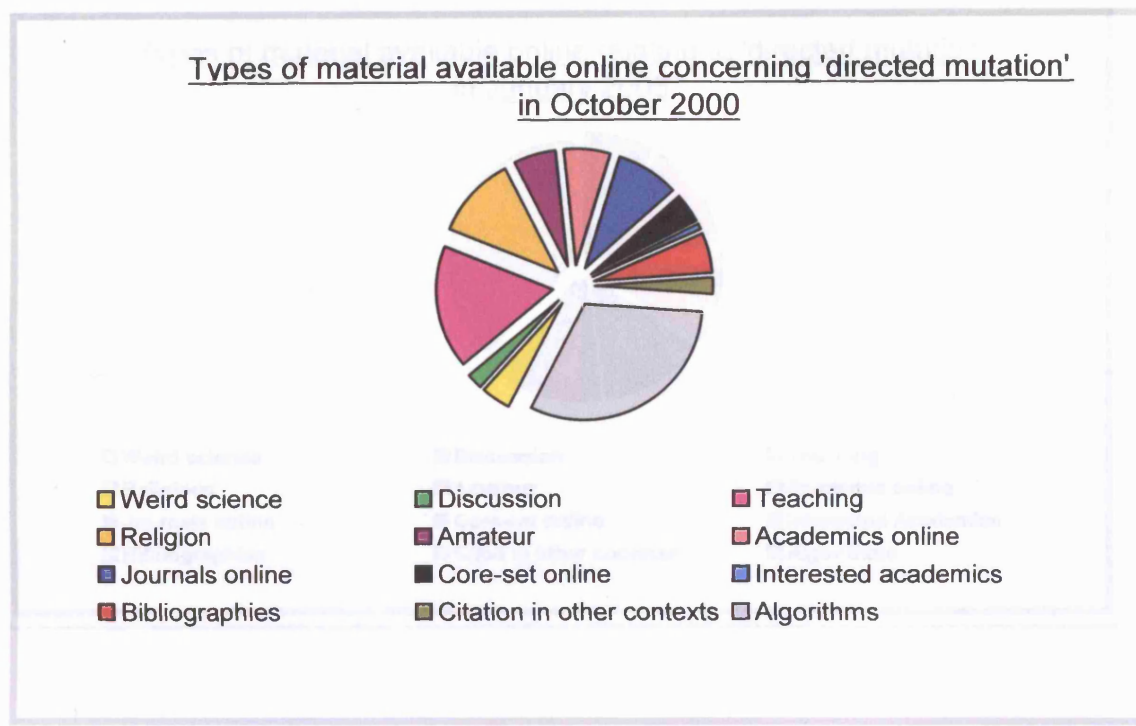


Figure 7

### Directed mutation on the Internet in 2005

In January 2005 a Web search, using the search engine Google.co.uk, and the search term 'directed mutation', yielded 747 hits. This is more than three times the number of hits in 2000. Figure 8 illustrates the proportion of the 2005 hits in each of the categories identified above. Figure 9 provides a comparison of the volume of materials available across the categories between 2000 and 2005. Again, references to site-directed mutation are excluded in each case.

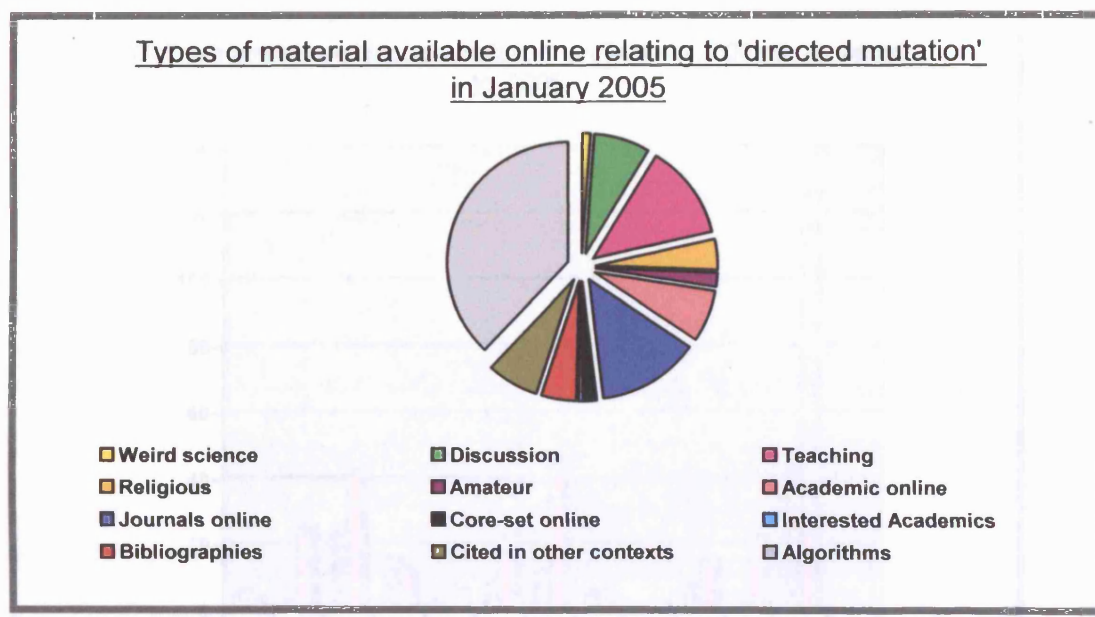


Figure 8

Figs. 7, 8 & 9 reveal the changing shape of online material relating to 'directed mutation' between 2000 and 2005. First, the data reveals a significant increase in the attention of media. With the 'info-bio-tech mutation' kit and web drag links excluded from the 2000 sample the number of hits was 1, across the entire sample. In 2005 the same search, with references and drag links excluded, yielded 212 hits. So the presence of directed mutation online had more than quadrupled in 5 years. This is not to suggest that this was a unique case unique to the case of directed mutation online. During the period 2000-2005 the overall scale of the Internet increased dramatically. In December 2000 there were 167,000,000 Internet users worldwide<sup>50</sup>. In December 2004 that figure had grown to 976,000,000 users

<sup>50</sup> NUA Internet survey. View online at <http://www.nua.ie>

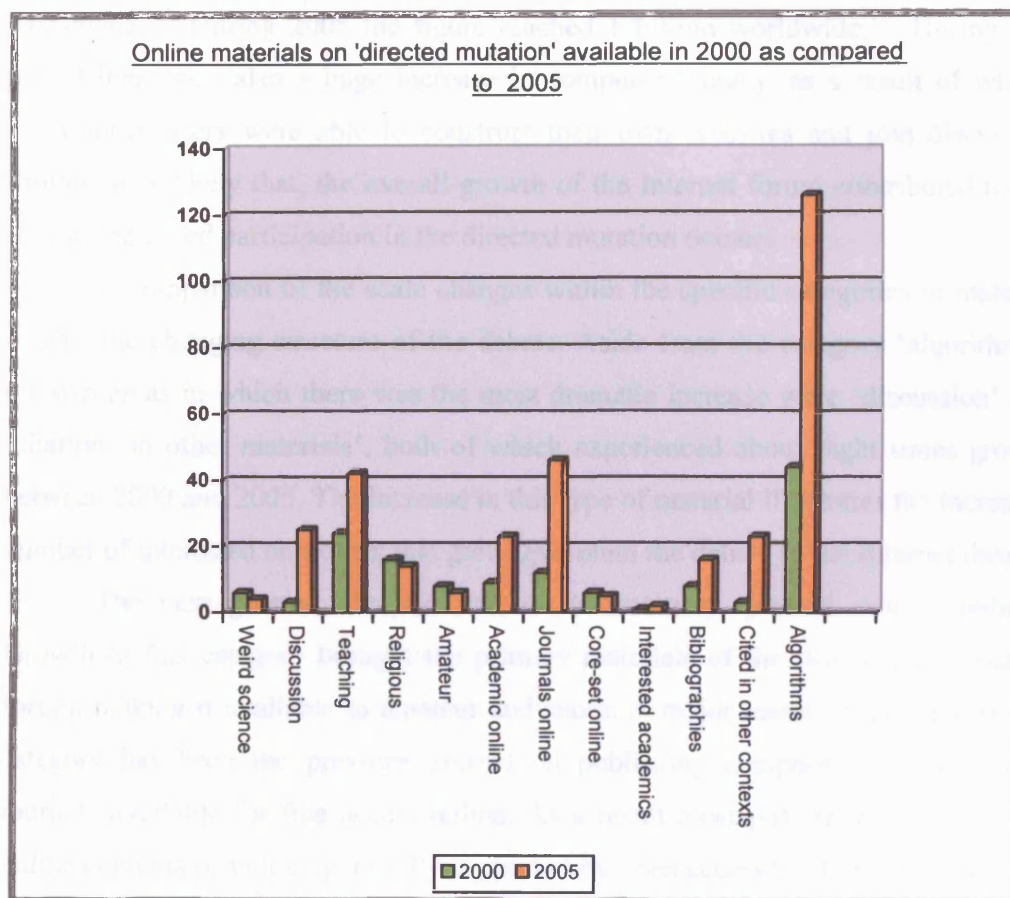


Figure 9

Figs. 7, 8 & 9 reveal the changing structure of the directed mutation debate in the Internet forum between 2000 and 2005. First, the data reveals a considerable extension of scale. With the 'site-directed mutation' hits and any dead links excluded from the 2000 sample the number of relevant, accessible hits was 140. In 2005 the same search, with 'site-directed mutation' and dead links excluded yielded 332 hits. So the presence of directed mutation online had more than doubled in 5 years. This is not to suggest that this scale increase was unique to the case of directed mutation online. During the period 2000-2005 the overall scale of the Internet increased dramatically. In December 2000 there were 163,000,000 Internet users worldwide.<sup>711</sup> In December 2004 that figure had grown to 934,480,000 users

<sup>711</sup> NUA Internet survey. View online at: [www.nua.ie](http://www.nua.ie)

worldwide.<sup>712</sup> During 2005 the figure reached 1 billion worldwide.<sup>713</sup> During that period there was also a huge increase in computer literacy, as a result of which, many more users were able to construct their own websites and join discussion forums. It is likely that, the overall growth of the Internet forum contributed to the overall increased participation in the directed mutation debate.

A comparison of the scale changes within the specific categories of material reveals the changing structure of the debate. Aside from the category 'algorithms', the two areas in which there was the most dramatic increase were 'discussion' and 'citations in other materials', both of which experienced about eight times growth between 2000 and 2005. The increase in this type of material illustrates the increased number of interested onlookers that grew up around the debate in the Internet forum.

The next greatest increase was in the category 'journal articles online'. Growth in this category brought the primary materials of the debate into a public forum, making it available to amateur audiences. A major cause of increase in this category has been the pressure exerted on publishing companies to make their journals available for free access online. As a result most publishers offer at least online contents or indexing, usually supported by open access to abstracts, if not full text. The key papers from this debate are all available online in full text; whether on publishers' websites, in teaching materials, or with commentary on amateur websites. The result of this increased access to the scientific discourse on directed mutation means that an interested audience could also become an informed audience.

Teaching materials and academic contributions online both experienced around 100% increase. This perhaps reflects increasing computer literacy in this period. Many academics have received training enabling them to produce web resources for their students, and online tuition has become a familiar part of most undergraduate programmes. The increase in 'academics online' perhaps illustrates that staff trained to produce student web resources have also gone on to use those skills to pursue their own interests.

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<sup>712</sup> Computer Industry Almanac. View online at: [www.c-i-a.com](http://www.c-i-a.com)

<sup>713</sup> Computer Industry Almanac.



The categories 'weird science' and 'amateur' have undergone slight decline. This is perhaps to be expected, since both these categories prize novelty and anomaly. After several years of debate the novelty of directed mutation has faded. The sensationalism of the first Harvard publication, and the immediate storm of dissent that it occasioned, have been replaced by the negotiation of the fine points of the molecular problem, in very technical papers, appropriate only to a narrow community of experts. The authors of these sites were interested in the Lamarckism versus Darwinism element of the debate, and since that aspect has largely been eclipsed in journal publications since the early 2000s there is not a great deal of new material for these dissent enthusiasts to engage. A constant stream of anomalies and unexplained phenomena arise, providing these organisations and individuals with new material. In that sense directed mutation has become old news.

The presence of the core-set online has also not increased. There are two possible explanations for this. Perhaps those authors consider that their journal publications speak for themselves, and feel no need to publish outside the parameters of peer review. Alternatively, those few authors might personally be ill-disposed to engaging in the Internet based debate. For example, John Cairns has said:

'I know nothing about any Internet forum on directed mutation (the idea fills me with horror).'<sup>714</sup>

Finally, religious or anti-religious attention has slightly decreased. The materials online in 2000 illustrate that there was no real consensus amongst these communities as to whether directed mutation was commensurate with or antithetical to religious approaches to science. For some it was evidence of design, for others it was another example of the inadequacy of evolutionary theory. With the relevance of directed mutation somewhat unclear to this community it has not been given further attention.

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<sup>714</sup> John Cairns (personal communication, 10/03)

Overall, there has been a general increase in the scale of the directed mutation debate online, with the greatest increase of materials in the categories linked to professional science, and academia. Many of the journal articles online originally appeared in print within the constitutive forum, and many online authors, for example those in the category 'academics online', occupy positions of authority in the cultural cartography of science outside the Internet forum. Although the signifiers of authority have a low visibility online (see section 5.3) nevertheless, many contributions to the online debate emerge from contexts that do confer authority in the standard cartography.

Supplementing those materials are the amateur contributions, which emerge from contexts that do not share the authority of 'science' outside the Internet forum. In the standard cultural cartography those individuals and contexts would exist outside the territory 'science', and they would not be part of negotiation during scientific dissent. However, in the Internet forum professional and amateur contributions appear side by side, and are often difficult to discern from one another. The implications of this blending of materials are discussed below.

## **5.2 New audiences generate new conflicts: is closure possible in diversified debates?**

In the period immediately after the publication of *The Origin of Mutants* the directed mutation debate was comprised of two sub-debates. This structure is described in Chapter 2. In that chapter I argue that the two-aspect structure perpetuated the conflict. The Lamarckism versus Darwinism aspect of the debate has been highly contentious, less openly negotiated in journals, and has made little progress towards closure. Meanwhile, the aspect of the debate that deals with the molecular biological phenomenon has been more explicitly addressed in the constitutive forum. However, without resolution of the Darwinism versus Lamarckism issue the molecular debate cannot achieve full resolution. In the late 1980s there were three major foci of dissent in this debate:

- Do bacteria have the capacity to control their mutational process and thus undergo adaptive change directed by environmental pressures?
- Would the existence of a mechanism of that kind represent a challenge to Neo-Darwinian theory?
- Would a mechanism of that kind provide an example of the action of Lamarckian evolution?

The first of these is open to negotiation and closure by common processes; for example, appeal to new evidence could potentially provide the route to consensus. Through experiment and negotiation this issue could potentially be resolved. The second and third issues are more difficult to resolve. The achievement of consensus on these questions requires more than the emergence of further experimental data. Allegiance either to a Darwinian interpretation or a Lamarckian interpretation of directed mutation is rooted in individual beliefs about evolution and evolutionary theory. Therefore, even prolonged negotiation is unlikely to achieve consensus on these issues because they are underpinned by a more fundamental disagreement. Many conflicts also reach a form of resolution through the process of abandonment. Again, in the case of these last two issues that is unlikely since the dissent is of such epistemological significance.

The uptake of the debate in the Internet forum enhanced these problems and added several more. Because most of the treatments online were secondary materials (i.e. commentaries) they did not offer the kind of new evidence that might lead to the resolution of molecular biological element of the debate. So, they did not contribute to the most likely route to resolution. Furthermore, most of the amateur treatments online focussed on the dissent between Lamarckism and Darwinism, inflaming this most contentious element. By framing the debate as a Lamarckian resurrection, amateur authors focussed attention on the aspect of the debate that was least likely to be resolved by appeal to evidence, abandonment or negotiation.

In addition to aggravating the existing impediments to closure, authors on the Internet also contributed further problems in the form of additional foci of dissent. Authors in the category 'weird science' encouraged that the directed mutation debate

also became about conspiracy and the repression of anomalies in science. They asked:

- Has the work on directed mutation been unfairly evaluated or rejected just because it is unorthodox?
- Is Neo-Darwinian theory defended in science beyond the degree warranted by the evidence?
- Are all findings that conflict with Neo-Darwinism marginalized?
- Does science protect its authority by rejecting anomaly?

For example, the author of *science-frontiers*, William Corliss, discusses ‘the seemingly unassailable dogma of evolutionary biology’, and asks regarding directed mutation: ‘can anything be more heretical?’ Corliss comments that:

‘This discovery [directed mutation] seems at least as “impossible” as the “infinite dilution” experiments discussed elsewhere. Will *Nature* now dispatch a “hit squad” to Harvard.’<sup>715</sup>

When Corliss revisits the directed mutation debate in a 1994 issue of *science-frontiers* he recalls that after the Harvard results were published ‘one of science’s foundation stones was at risk’ and adds ‘this claim was too awful to accept’.<sup>716</sup>

In addition, authors who contributed materials with an explicit religious or anti-religious agenda encouraged that directed mutation become involved in the evolution versus creation debate. They asked:

- Does directed mutation provide evidence of intelligent design?

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<sup>715</sup> Corliss is making reference here to the reception of Benveniste’s work on water memory, which was published in *Nature* (Davenas, Benveniste *et al.*, 1988) just before *The origin of mutants* (Cairns, Overbaugh & Miller, 1988). The ‘hit squad’ that he mentions is a reference to the deputation that *Nature* editor John Maddox led to Benveniste’s laboratory to examine his experimental procedures (see 5.6). See *Science Frontiers* #60, Nov-Dec, 1988. View online at: [www.science-frontiers.com](http://www.science-frontiers.com)

<sup>716</sup> *Science Frontiers* #96, Nov-Dec, 1994. View online at: [www.science-frontiers.com](http://www.science-frontiers.com)

- Does directed mutation provide evidence that orthodox evolutionary theory is flawed at a fundamental level?

Again, these issues are not open to resolution by means of appeal to new data, re-negotiation of existing evidence or abandonment.

In the Internet forum directed mutation developed various identities in relation to new agendas. Consensus was made even more unlikely because authors in each category were not necessarily interested in the others' commentaries. For example, academic authors online were not necessarily moved to engage in the religion versus science debate. The religious authors were similarly disinterested in the possible conspiratorial marginalisation of Lamarckians. Negotiation was not being achieved because the numerous authors did not share an interpretation of the dissent.

Ultimately, closure depends on the achievement of a significant degree of consensus amongst those engaged in a conflict. In the case of directed mutation on the Internet, consensus could not even be reached concerning the identity of the dissent implied by the directed mutation phenomenon. The debate had diversified in this forum to become several related, yet quite different, sub-debates; each requiring its own form of closure. Different communities have different approaches to and identities of dissent and resolution, making negotiation between groups difficult. Chapter 4 identifies the clash of the sub-disciplinary groups 'evolutionary biology' and 'molecular biology' as a force of perpetuation in the directed mutation debate. In that chapter I argue that perpetuation resulted from these two groups pursuing different negotiation strategies, and having different understandings of closure. In the Internet forum, these two clashing communities were joined by several others, and the clash of styles of approach was magnified and multiplied.

### **5.3 Is there boundary work in the virtual world? What becomes of the cartographic metaphor online?**

Chapters 1–4 highlight the broad utility of boundary work theory as a methodological tool for the study of the directed mutation debate. Chapter 3 describes how boundary theory reveals the debate as one episode within a larger conflict. The theory allows that we frame the identities of Lamarckism and Darwinism as ‘old maps’, and understand their histories accordingly. Chapter 4 describes how boundary work and cultural cartography illuminate the circumstances of the interdisciplinary contest between molecular biologists and evolutionary biologists at the heart of the directed mutation debate. However, when we follow the debate into the Internet forum the success of boundary theory as an analytical tool is reduced.

Boundary theory relies heavily on the cartographic metaphor, with the concept of adjacency underpinning the notion of ‘boundary’. Representations of boundaries, and their associated authority signification, exist as part of a two-dimensional geographic metaphor. Gieryn calls these ‘culturescapes’.<sup>717</sup> The 2-D cartography that records the episodic delineations of territories illustrates a transient consensus between the adjacent territories regarding the location of a boundary. Some visual representations of these 2-D cultural cartographies have been prepared. They record the proximity of certain cultural territories, illustrating adjacency and depicting their transient boundary delineations. ‘The Map of a Great Country’ is one example. This 1834 map produced by an American artist was intended as an allegory on alcoholism; depicting intemperate behaviours in the lands of ‘poor prospect’ and ‘direness’ beside the seas of ‘anguish’ and ‘perdition’. In the south of the map, across the sea of temperance we see the states of ‘knowledge’ and ‘fine prospect’ where ‘Mount Science’ can be found.<sup>718</sup>

Although adjacencies and associated authority are open to constant debate, nevertheless, the geographic metaphor can be used to illustrate the state of cultural delineations at any one time. The boundaries on these maps depict the sites at which conflict occurs, and the scale of the territories demonstrates the authority of each cultural domain in relation to its ‘neighbours’. In boundary work theory these

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<sup>717</sup> Gieryn, T. (1999) p.7

<sup>718</sup> This map has been reproduced in Gieryn, T. (1999)

cartographies, the boundaries they depict, and the authority that they record, are assumed to reflect the tacit assumptions of the members of the cultural territories. Those tacit assumptions are used to navigate culture. In these geographic cartographies there are certain 'signposts' or signifiers that enforce assumptions of authority and delineation. For example, in the case of science, the territory is signposted by symbols such as universities, professional journal publications, professional conferences, citations, and even white lab-coats. The signifiers are vital parts of the cartography, and they guide activity both in culture and in new boundary disputes.

However, when we come to consider the representations on the Internet things are dramatically different. It is unclear how the cultural cartography can be pasted onto this new space; how the geographic metaphor might be applied to cyberspace. Also, it is unclear how the cultural cartography's boundaries, adjacency and authority are replicated in this environment. There are two principal features of the Internet that make it so incommensurable with the cartographic metaphor and boundary theory: first, the physical organisation of the loci (i.e. websites or web pages) within the overall space, and, second, the manner in which navigation through the space and its constitutive loci is achieved.

In cyberspace there is no reality of adjacency between the various sites. The user creates a fresh adjacency with each navigation. There is no pre-determined route between loci and no linearity linking them. The start and end points of any users navigation are unfixed, except in as much as they might explore loci using the navigation order generated by the listing processes of a web search tool. The navigation or 'surf' of any user will be absolutely episodic and transient, and the navigational route followed by any one user does not influence the path that subsequent users will take.

Furthermore, the transient loci relations in this space are not supplemented by the kinds of signifiers used in the cultural cartography; in the Internet forum, association, authority and credibility are not signposted by the artefacts or indicators that are visible in the constitutive or contingent forums. For example, away from cyberspace, peer review marks journal articles as professional, and university

affiliation marks authors as experts. In the Internet forum it is not necessary to demonstrate authority in order to present material, and the usual boundaries and associated signifiers are no longer functional or even relevant.

However, the Internet forum is not totally without a system of navigational guidance between loci. In that environment hyperlinks provide pointers for individuals' navigation, and search engines provide a kind of recommended route through materials. However, even where links are used, navigations remain highly variable, and searches still rely on the user to choose which loci to visit from the filtered selection. Links and searches provide a guide to subject, but not to authority. Journal publications citations are the analogue of links. However, in journals citations imply a navigational aid related to the pursuit not only of subject but also of authority. Citations are used to assert community, and highlight the shared authority within a certain cultural territory. Citations can be a tool of boundary work and can imply inclusion or exclusion. Links between web loci do not generally fulfil such a sophisticated role, rather they offer a very basic navigational guide, prioritising subject over affiliation or authority. As business economist Detlef Schoder points out: 'In the physical world two-dimensional maps and various other symbolic representations of our environment are the preferred means of orientation.' But that by contrast, on the Internet: '...the navigational support provided by links, or a collection of links, is rather limited...[they] resemble street signs, and as such, are not that helpful for high-dimensional cyberspace navigation'.<sup>719</sup>

In some cases the use of hyperlinks fulfils a similar function to citation in paper publication. Academics online tend to link to other academics and to online journal material, rather than linking to the online contributions of amateurs. Those authors are using links to replicate the authority structures in the cultural cartography. Amateur authors on the other hand include links to professional and amateur materials, choosing to prioritise subject over authority. Furthermore, paper publications rarely cite online materials, whereas online materials very frequently

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<sup>719</sup> Schoder, D. (1999) Navigation in cyberspace – using multi-dimensional scaling to create three-dimensional navigational maps. In: W. Gaul & H. Locarek-Junge (Eds.) *Classification of the information age*. Proceedings of the 22<sup>nd</sup> Annual GfKI. Conference, Dresden. Available online: [www.iig.uni-freiburg.de/telematik/forschung/publikationen/pubfiles/Sc1999a.pdf](http://www.iig.uni-freiburg.de/telematik/forschung/publikationen/pubfiles/Sc1999a.pdf)



refer to paper publication. It is possible that a detailed analogy could be drawn between the use of citations and online links. That analogy might allow the choice of links to be viewed as a kind of boundary work online, and this would certainly be an interesting area for further research.

In spite of the apparent lack of adjacencies on the Internet, attempts have been made to provide a cartography of cyberspace and the navigations made within it. Much of this attention has been inspired by business interests, with companies keen to locate and link their sites in a way that makes visiting them both more simple and more likely. There are also 'cyberspace geographers' whose interest in creating a cartography is inspired by the spirit of exploration. They are attempting to map the new world that cyberspace represents. Examination of some of the maps generated through these efforts highlights the difficulty of assigning concepts such as adjacency or the geographic metaphor in this environment. Some representations use the principles of existing cartography, while others rely upon new metrics, grids and abstractions.

Computer scientists Martin Dodge and Rob Kitchin have made perhaps the most comprehensive study of the new cartographies of cyberspace. They have categorised the cyberspace cartographies and collected images in an 'Atlas of Cyberspace'.<sup>720</sup> The visual representations of cyberspace take several basic forms. Firstly, there are those representations that attempt to salvage the usefulness of the geographic metaphor. These use standard cartographies as their foundation and attempt to paste a representation of cyberspace over those familiar images.

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<sup>720</sup> Kitchin, R. & Dodge, M. (2002) *Atlas of cyberspace*. Addison-Wesley, London.



Figure 10: Visualization study of the NSFNET<sup>721</sup>

There are also conceptual maps of cyberspace. These attempt to salvage the concepts of adjacency and scale and preserve these by depicting the web as if it were physically arranged in space. Perhaps the best known of these is the Internet Industry Map, which charts cyberspace by depicting the relationships between the major enterprises that are hosted in cyberspace.



Figure 11: The Internet Industry Map<sup>722</sup>

<sup>721</sup> Visualization produced by D. Cox and R. Patterson 1992

<sup>722</sup> Visualization of cyberspace in terms of the commerce that it hosts. Prepared by Valdis Krebs in 2000. The most recent version of the Internet Industry Map is available online: [www.orgnet.com/netindustry.html](http://www.orgnet.com/netindustry.html)

Other representations of cyberspace tend towards the abstract, and actively attempt to escape the restraints of standard cartographies with their implicit categories of adjacency and scale. At their most abstract these 'cartographies' become artistic representations of the abstraction of cyberspace.



Figure 12: 3-D Internet topography visualizations, created using Walrus visualisation software.<sup>723</sup>

Some of the most abstract visualisations exist in film, where cyberspace is given a physical manifestation as another 'world'; often a world with enough constructed physical reality that people can 'go there'.



Figure 13: Still from the film 'The Matrix' in which the Internet is depicted as a physical environment manifest from streaming data.

Figure 14: Still from the film 'Johnny Mnemonic' in which the Internet is depicted as a physical environment manifest from the data stored in cyberspace.

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<sup>723</sup> Walrus visualizations of cyberspace presented in Kitchin, R & Dodge, M. (2002)

In addition to these cartographies, or visualizations, of the overall structure of cyberspace, there are also cartographic representations and visualizations of the individual navigations of any one user, at any one time, through the environment. These cartographies record the transient adjacencies that are created by each user's personal navigation.



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Figure 15: Basic surf-map showing a simple navigation.

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Figure 16: A Natto view 3-D visualization surf-map.



Figure 17: This surf map is a WebPath visualisation, created by a mapping tool that visualizes and records a user's trail as they browse the Web.<sup>726</sup>

<sup>724</sup> Surf map created using WebMap, a graphical navigation visualization tool. Produced by Peter Domel, 1994.

<sup>725</sup> Natto View visualization has been developed by H. Shiozawa and Y. Matsushita.

<sup>726</sup> WebPath visualization software has been developed by E. Frecon and G. Smith.

However, while these 'surf maps' might record transient adjacencies, they cannot be used in the same way as the adjacencies in the cultural cartographies i.e. to illustrate boundaries or associated authorities. Without the signifiers of the standard cultural cartographies authority and association become less visible. As you navigate between sites the signifiers of authority are discreet. Also, search engines do not discriminate in terms of authority, and so provide no instructive guide as to the validity of sources.

In the case of the materials related to directed mutation there is a mixture of professional and amateur material. There are tacit and explicit agendas being served on many of the sites. In some cases the material presented is even factually incorrect. There are some ways of identifying remnants of the authority structures of the standard cultural cartography. For example the '.ac' or '/edu' in a web address signifies university affiliation and implies the authority association that attends that status in the cultural cartography. Also, the user might benefit from having existing knowledge of the authority relations within a certain topic, for example they might recognise the names of individuals or authors that have authority in the context of the cultural cartography.

It is difficult to imagine how boundary work might be visualised, or even manifest, in this environment. It seems that the cartographic metaphor cannot be applied, and thus adjacency and the delineations that boundary theory relies upon are lost. Science communications researcher, Adam Nieman, has considered the possibility of reinterpreting boundary work without the cartographic metaphor.<sup>727</sup> Nieman rejects the cartographic metaphor in favour of a 'network model', in which boundaries are determined by the 'intermediate dependencies' of the various territories. It is possible that this kind of network model might be applied to illuminate the transient adjacencies between loci that are created by the navigation of web users. The relationship of the loci one to another during that navigation would be an example of an intermediate dependency. However, this still cannot reveal

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<sup>727</sup> Nieman, A. (2002) Boundary negotiations in popular culture: 'intermediate dependent entities' and the ideological context of science policy [Preprint].

boundary work activity because those brief interdependencies of adjacencies are transient, and specific to one user. The consensus required for boundary work to function is lost in the temporary nature of the adjacencies. It seems that the authority relationships online will require an alternative to the cartographic metaphor for their description.

#### **5.4 Is the Internet forum ‘contingent’ or ‘constitutive’? A new arena for acceptance and rejection?**

Harry Collins and Trevor Pinch have identified the constitutive and contingent forums as the contexts in which scientific controversy is conducted (see Chapter 1).<sup>728</sup> They characterise the constitutive forum as ‘scientific theorising and experiment and corresponding publication and criticism in the learned journals and, perhaps, in the formal conference setting.’<sup>729</sup> The contingent forum on the other hand is comprised of ‘those actions which – according to old-fashioned philosophical orthodoxy – are not supposed to affect the constitution of ‘objective’ knowledge.’ In particular ‘...popular and semi-popular journals, discussion and gossip, fundraising and publicity seeking, the setting up and joining of professional organisations, the corralling of student followers, and everything that scientists do in connection with their work, but which is not in the constitutive forum.’<sup>730</sup>

It is difficult to assign the online directed mutation materials to these categories. They include contributions from both amateurs and professionals. Some of the materials are reproduced from the constitutive forum, for example, the many journal articles that have been made available. Other materials are more like those in the contingent forum, for example, non-reviewed online academic publications and personal communications in online discussion groups. The Internet provides the arena for professionals and amateurs to discuss science issues without the restrictions of highly visible authority signifiers. Furthermore, it allows amateurs to

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<sup>728</sup> Collins, H. & Pinch, T. (1979)

<sup>729</sup> Collins, H. & Pinch, T. (1979) p.239-240

<sup>730</sup> Collins, H. & Pinch, T. (1979) p.240

comment on the reproduced constitutive materials and make their commentary visible to authors from the constitutive forum. The mixing together of these materials, alongside the blurring of the authority signifiers, makes it difficult to discern the contingent and constitutive categories. Part of what coheres the constitutive and contingent is the boundaries and associated authority that delineate them one from the other. As described in 5.3 these boundaries, adjacencies and authority structures breakdown on the Internet. As boundary work looses some of its descriptive power in the online environment, so to do the categories contingent and constitutive.

I suggest that it is not useful to assign the online materials to these forums. If we view the Internet simply as an assemblage of the contingent and constitutive then the particular character of this unique space for scientific debate is eclipsed. The Internet provides the opportunity for amateurs and professionals to engage together, even if not with one another, in the negotiation of scientific controversy. It promotes openness, not only in the author's ability to make their opinions visible, but also in the availability of sources. In this environment the restraints of peer review are bypassed, and negotiation becomes a more lively and immediate process. In place of the long pauses between assertion and response that are occasioned as authors submit papers, wait to have them accepted and for journals to come into print, the Internet offers a context for more spontaneous discourse. For these reasons, I argue that it is useful to think of the Internet forum as a third arena for scientific controversy; existing alongside the contingent and constitutive forums, sharing some of each of their characteristics, but having an identity particular to it.

However, since the usual boundaries and signifiers are not visible in the context of the Internet, we must ask whether it is valid to acknowledge the Internet forum as a site of scientific debate. If the rules that the cultural cartography enforces concerning authority are not applied in this context, then can any negotiation that happens there be fed back into a system that makes choices about boundaries and authority? That is to say: is debate on the Internet part of a scientific controversy, and should it have an impact on negotiation in the contingent and constitutive forums?

The data in fig.9 indicates the validity of Internet materials as part of the directed mutation debate. Two of the fastest growing categories of online material between 2000 and 2005 were 'academics online' and 'journal articles online'. By contrast the categories 'amateur online' and 'weird science' experienced slight negative growth. So, the increased scale of directed mutation online in 2005 mainly resulted from an increase of materials in the more professional of the categories. The amateur materials, which often provoke concerns about the validity of online debate, represented less of the online materials overall than the professional in 2005. In the case of directed mutation at least, the notion that online materials are invalid because they are unprofessional seems inappropriate.

We learn from boundary theory that authority is negotiated between the cultural territories. A boundary is delineated at an agreed location between adjacent groups. A group cannot get authority by just saying that they have it, or demanding it – it must be agreed. This highlights an interesting role for the Internet, and emphasises its importance as a site for debate; as long as the Internet community is engaged in debate the boundaries and authority are not set since consensus across the cartography has not been achieved. Although authors in journals might conclude on the authority contest, while others from adjacent territories remain undecided this cannot be finalised. Although there is concern about what the Internet discourse 'adds' to debate in terms of intellectual material, that concern really misses the point. Material on the Internet might not be sufficiently intellectual or professional to occupy the constitutive forum, but it does show that individuals from other cultural domains don't accept the constitutive decrees on closure. And without their agreement boundaries cannot really be set.

Interestingly, the Internet reveals boundary activity between communities. If we just look at journal articles all we see is the boundary work of authors as they push for increased authority. The Internet allows us see their opponents, and watch the boundary negotiation as a crowd of non-scientists or non-professionals ask questions about validity and fairness before agreeing on boundary and authority outcomes. The traditional problem of what the Internet 'adds' to scientific debate can be changed to a question about what the Internet 'does' to scientific debates.



In terms of the protraction of debate, with which this thesis is concerned, activity in the Internet forum has had an undeniable influence. In the case of directed mutation, the Internet has not only increased the scale of debate, but it has also encouraged its diversification of identity. As mentioned above, the debate would now require several different closures, pertaining to the different interests and agendas that are involved, to be considered closed by consensus. The Internet forum, with its absence of controls such as peer review, has allowed the Lamarckism versus Darwinism debate to come to the fore. I have identified that element of the debate as a principal force of perpetuation, even in the paper debate where reference to that conflict is necessarily less explicit. With the Internet providing a site for negotiation of that aspect of the debate, perpetuation is enhanced. Although authors in the constitutive forum are less willing to engage that aspect, nevertheless, debate is perpetuated by having its most controversial element catered for in a new space. Therefore, it is not only interesting to consider the distinct nature of the Internet forum, but in this case it is a pragmatic necessity for achieving an understanding of the debates dynamics. I will not seek to further characterise the nature of the Internet here (that must be a larger project for attention elsewhere), but suffice to say, that I will assume it as a third distinct context for debate alongside the constitutive and contingent forums.

### **5.5 What becomes of ‘safe discredit’ in the twenty-first century? The implications for closure of contemporary conflicts.**

In 1995 evolutionary biologist Daniel Dennett pronounced directed mutation dead. He stated that the theory had been ‘safely discredited’ and rejected by the scientific community.<sup>731</sup> The dozens of journal articles published relating to directed mutation subsequent to 1995 indicate that Dennett’s evaluation was premature. Furthermore, the huge growth of the debate in the Internet forum between 2000 and 2005 illustrates that negotiation is far from concluded. Even if Dennett’s commentary had been true of the constitutive debate, it still would not have reflected the

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<sup>731</sup> Dennett, D. (1995)

circumstances of uptake in the Internet forum. In this section, I argue, that notions of discredit and unorthodoxy loose meaning in debates taken up in the Internet forum. These debates achieve a virtual immortality, and closure becomes a remote possibility. Without the consensus that underpins closure, debates are potentially protracted indefinitely.

Sociologist Bart Simon has described what he calls the ‘afterlife’ of scientific controversies in relation to the cold fusion debate.<sup>732</sup> He defines the afterlife as the existence of a debate in what appears as a ‘post-closure’ incarnation, and has developed a methodology for examining controversies that endure, despite apparent closure in the constitutive forum. He calls this the ‘hauntology’ of undead science. Simon has observed that after the scientific community rejected cold fusion there was in fact little change in the number of people working on the phenomenon, or interested in it. What changed was the way that those individuals had to conduct their research. After 1990 supporters of cold fusion worked more discretely, often in privately funded research institutes created for the purpose, and often publishing in specialist journals created by and for this community of researchers. Despite the fact that their structures of funding and publication changed, the material of the debate remained unchanged. Although many scientists had declared the end of the controversy, even branding cold fusion ‘pathological’ or fraudulent, if we follow the object of cold fusion into other contexts (as Latour and Simon have encouraged) we discover that the debate survived and continued unperturbed by the pronouncement of its closure.

In this section, I argue that the Internet is an ideal and common place for ‘undead’ scientific controversies to be conducted. I have suggested in 5.3 that boundaries and authority do not translate well in the context of this forum. Here I argue that the concept of closure also is less relevant in the Internet forum, allowing many controversies to exist in a post-closure afterlife of the kind that Simon has identified. If there are undead controversies, as Simon suggests, then the Internet is an ideal realm for the dead; a hinterland between rejection and abandonment.

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<sup>732</sup> Simon, B. (2002)

In the case of cold fusion, the Internet has allowed the debate to undergo full resurrection. Although the orthodox community have abandoned the debate, it is able to continue aside from that rejection in the Internet forum. A similar situation is illustrated in the case of the water memory debate (see section 5.6). That debate was declared closed in the autumn of 1988<sup>733</sup>, yet this did not prevent further research and negotiation continuing in other contexts. The water memory debate, like the directed mutation debate, has taken on numerous meanings in relation to the agendas of various interested groups. The diverse negotiations between and among those groups can be described as the afterlife of the water memory controversy. It seems that in the case of both cold fusion and water memory the process of 'discredit' has not had the impact expected. Although one community has rejected the claims, and asserted that it has the authority to do so, nevertheless others who contest that authority continue to negotiate and hold that rejection in little regard. And since cultural authority is negotiated between groups, and cannot just be declared by the interested party, ongoing debate is enough to leave boundaries undecided.

The situation as regards directed mutation is slightly different. Consensus concerning the status of directed mutation has not been reached in any forum. While Dennett has pronounced the theory dead, many other scientists continue to research the area and engage in the paper debate. Although many scientists encourage a Darwinian (or at least non-Lamarckian) interpretation, still research also continues that supports the 'stronger' versions of directed mutation theory. The uptake of directed mutation in the Internet forum has not necessarily meant its reincarnation; directed mutation has essentially not been declared dead and so is not quite 'undead' in Simon's terms. However, the increase of scale in the Internet forum has certainly breathed new life into the debate. The numerous new identities of directed mutation in that context mean that the debate is enjoying a kind of afterlife analogous to the cases of water memory and cold fusion online, but is simultaneously alive in the constitutive and contingent forums.

I have argued that the directed mutation controversy is comprised of two sub-debates: the first being the negotiation of the anomalous molecular biological

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<sup>733</sup> Maddox, J. (1988b)

findings, the second being the negotiation of the implications of that anomaly for Lamarckian and Darwinian theories. By the late 1990s it was the negotiation of the molecular biological elements of the debate that took precedence in the journal treatments. That was largely due to an awareness that the Lamarckism versus Darwinism element of the conflict could not be resolved by debate, and would be best addressed by finding further data related to the directed mutation phenomenon. In the late 1990s, the journal articles increasingly began to address specific technical issues relating to directed mutation, rather than the general implications of the phenomenon. Speculative or antagonistic commentary concerning the Lamarckian implications of directed mutation became far less common in print. Although the Lamarckian element of the debate had not been resolved it was being excluded to a degree from the constitutive materials based upon its apparent irresolvability. However, the uptake of the debate in the Internet forum allowed a resurrection of the active negotiation of that element of the debate, and in fact, that level of the debate took precedence on the many amateur sites. So although one level of the debate remained very much open in the constitutive forum, the more controversial elements of the debate began to enjoy an afterlife and resurrection away from the restrictions of the paper debate.

The existence of the Internet forum, as a context for certain debates to conduct their 'afterlife', or at least enjoy some enlivenment if they are not yet dead, means that the very notions of discredit and rejection in science are changed in meaning. Discredit in science is only influential if there is a degree of widespread consensus concerning the legitimacy of the discredit. The Internet provides a forum in which individuals or groups that do not accept the evaluation of orthodox scientists can continue to negotiate scientific issues. The great scale of engagement on the Internet allows discredit to lose its influence in the online environment. Closure relies upon the achievement of consensus regarding a contentious issue. The consensus does not have to be unanimous, but must be of a degree that means negotiation comes to an end. On the Internet, the degree of participation, and the different interests and agendas that underlie participation, make the achievement of consensus a practical impossibility. The issues of contention diversify such that there

is not even consensus regarding the nature of the controversy. The different groups or individuals do not all share values concerning authority structures, or what counts as orthodox versus unorthodox. So, notions such as ‘discredit’ do not carry weight in the new forum. Their relationship to the quasi-object that the debate represents is altered by the change of context, and we must take that into account as we follow the debate into different contexts.

## **5.6 An analogous case: Jacques Benveniste and the Homeopaths.**

### **Jacques Benveniste and water’s memory.**

In June 1988, an article appeared in *Nature* from the French laboratory of eminent immunologist Jacques Benveniste.<sup>734</sup> The authors of that paper reported that a water-based solution containing an antibody retained its capacity to evoke an a cellular response even once it has been so extremely diluted that there was unlikely to be even one molecule of the antibody remaining in a sample. They claimed that the water in the solution was somehow altered in an enduring way by that relationship with the antibody, even after dilution meant that no molecules of the antibody remained. The water behaved as if the antibody was still present, essentially the water seemed to ‘remember’ its presence.

Publication in the journal *Nature* of findings with such an apparent lack of physical basis was unusual. The journals editor John Maddox had approached the material with great caution. The paper had been sent in several versions to a number of referees over a period of months. When the paper finally appeared in June 1988 it was accompanied not only by an editorial introduction<sup>735</sup>, but also by a highly unusual ‘editorial reservation’.<sup>736</sup> In the editorial Maddox described the ‘good and particular reasons why prudent people should, for the time being, suspend judgement.’<sup>737</sup> He stressed that: ‘...when an unexpected observation requires that a

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<sup>734</sup> Davenas, E., Benveniste, J. *et al.* (1988)

<sup>735</sup> Maddox, J. (1988a)

<sup>736</sup> Maddox, J. (1988c) Editorial reservation. *Nature*, 333: 818.

<sup>737</sup> Maddox, J. (1988a) p.787

substantial part of our intellectual heritage should be thrown away, it is prudent to ask more carefully than usual whether the observation may be incorrect.’<sup>738</sup> In the appended editorial reservation Maddox revealed that one of the agreed conditions for publication was that Benveniste allow a delegation to visit his laboratory to examine experimental procedure. The report of that investigation was scheduled to appear in the next issue of *Nature*.

The publication of Benveniste’s work alongside these editorial contributions provides an excellent example of what Collins and Pinch have called ‘diluting orthodox publication’.<sup>739</sup> In this style of publication a paper is qualified by accompanying material, such that ‘the seeming stamp of legitimacy [is] emasculated by making it a case of special treatment’.<sup>740</sup> The result is that the publication becomes ‘at best a kind of tokenism’.<sup>741</sup> Collins and Pinch have described this type of publication in relation to an article on psychical research in the journal *Nature* in 1974.<sup>742</sup> In that instance, a paper reporting tests carried out on several alleged psychics, including Uri Geller, at California’s Stanford Research Institute, was published alongside an editorial explaining the publisher’s sceptical approach to the report.<sup>743</sup> The leader was written by a well-known sceptic of parapsychology, and included extracts from unfavourable peer-reviews. The editorial stated that *Nature* had published against the recommendations of the reviewers for several reasons. These included: i) because the research emerged from a reputable institution, ii) because the method used was scientific, iii) because there was a lot of media attention and rumour surrounding what had been achieved in the research and the editors felt they should provide actual data and iv) because the readers of *Nature* expect the journal to occasionally handle high risk or contentious material.

Benveniste’s paper was treated in a similar way. It was also introduced by editorial material that referred to unfavourable reviews, and the editor justified publication on the grounds of settling rumours that had begun. In Chapter 4, I

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<sup>738</sup> Maddox, J. (1988a)

<sup>739</sup> Collins, H. & Pinch, T. (1979) p.258

<sup>740</sup> Collins, H. & Pinch, T. (1979)

<sup>741</sup> Collins, H. & Pinch, T. (1979)

<sup>742</sup> Targ, R. & Puthoff, H. (1974) Information transmission under conditions of sensory shielding. *Nature*, 251: 602-607.

<sup>743</sup> Davies, D. (1974) Editorial: Investigating the paranormal. *Nature*, 251:559.

describe how Cairns' *On the Origin of Mutants* was similarly published alongside qualifying material, in that case a leader article from molecular biologist Franklin Stahl<sup>744</sup>, as a form of dilution of orthodox publication.

The qualification of Benveniste's research went further. The agreed laboratory visit went ahead, and John Maddox led a team to France. The composition of the team was remarkable, and further highlighted the publisher's scepticism. Maddox took with him James Randi, a famous magician and fierce antagonist of the paranormal, and Walter Stewart, a well-known investigator of fraud in science. They spent a week at the laboratory analysing experimental methods and data interpretations, and Randi and Walters searched for every kind of trickery or fraud.<sup>745</sup>

Their report appeared in the next issue of *Nature*, calling the high dilution experiments 'a delusion'.<sup>746</sup> The authors stated that Benveniste's experiments were ill-controlled, full of systematic error, fraught with observer bias and not reproducible. They did not accuse Benveniste of fraud, but rather stated that the researchers had seemed convinced of the reality of their reports. They concluded that: '...the claims made by Davenas *et al.* are not to be believed.'<sup>747</sup> The team of investigators suggested that Benveniste withdraw his published paper, or at least write to *Nature* to qualify his data and interpretations himself.<sup>748</sup> Benveniste refused to take that action. He did however write to *Nature*, but did so to offer a damning condemnation of the team that had visited his lab.<sup>749</sup> He likened the experience to 'Salem witchhunts or McCarthy-like prosecutions'. He enjoined scientists as his peers to 'never, but never let anything like this happen – never let these people get in your lab.'<sup>750</sup> Three months later *Nature* published Benveniste's reflections on his treatment.<sup>751</sup> He accused *Nature* of setting him up, and constructing his work as

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<sup>744</sup> Stahl, F. (1988)

<sup>745</sup> Maddox, J., Randi, J. & Stewart, W. (1988)

<sup>746</sup> Maddox, J., Randi, J. & Stewart, W. (1988)

<sup>747</sup> Maddox, J., Randi, J. & Stewart, W. (1988) p.289

<sup>748</sup> Maddox, J., Randi, J. & Stewart, W. (1988)

<sup>749</sup> Benveniste, J. (1988a) Dr Jacques Benveniste replies... *Nature*, 334: 291.

<sup>750</sup> Benveniste, J. (1988a)

<sup>751</sup> Benveniste, J. (1988b) Benveniste on the Benveniste affair. *Nature*, 335: 759.

fraud without supporting evidence. He stated: 'fact twisting, errors, omissions, misquotations and mistruths are symptoms of a crusade'.<sup>752</sup>

The result of the water memory affair was that Benveniste, a formerly highly regarded scientist 'was pilloried, losing his government funding and laboratory'.<sup>753</sup> As far as authors in the constitutive forum were concerned the affair was ended, and the conclusions drawn by the *Nature* team led the consensus of the scientific mainstream. In October 1988, Maddox presented a final editorial on the water memory saga, in which he stated that 'from this issue of *Nature*, the past several weeks' correspondence on the Benveniste affair will be closed.'<sup>754</sup>

Benveniste, and a depleted group of supporters continued the research undeterred, eventually under the auspices of the company Digibiol that Benveniste set up to investigate the potential applications of water memory. The persistence of that group of advocates meant that a degree of attention in the contingent forum was retained into the 1990s. And even in the constitutive forum water memory was not as 'closed' as Maddox had stated. In 1993 repetitions were attempted at University College London, showing negative results. However, in the early 2000s a pan-European project began to retest the basophil system with faultless controls, and researchers were 'surprised' to find significant positive results.<sup>755</sup>

However, the greatest forum for water memory debate from 1990-present has been online. A thriving community has made original sources and new publications available on various websites. Negotiation of the implications of water memory are carried out in dozens of online discussion forums. Like the directed mutation debate, the water memory debate online has achieved a huge scale and has been translated to suit many new agendas. In the case of water memory the Internet has extended the life expectancy of the debate. The transfer of the debate to that forum has allowed it to survive closure, and in fact become perpetuated in many new incarnations. The water memory debate is now in the online 'afterlife', and has a chance for

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<sup>752</sup> Benveniste, J. (1988b)

<sup>753</sup> Milgrom, L. (1999) The memory of molecules. *The Independent*, 19.03.99.

<sup>754</sup> Maddox, J. (1988b)

<sup>755</sup> Milgrom, L. (2001) Thanks for the memory. *The Guardian*, 15.03.01. In this article Milgrom reminds us how controversial water memory remains, stating that as a result of the positive findings in 2001 'Either Benveniste will now be brought in from the cold, or...the scientists involved in the pan-European experiment could be joining him there.'



immortality. An analysis of the nature of its afterlife gives an impression of just how successful Internet resurrections can be.

### Water memory online

In February 2005 a basic Web search, using the search engine Google.co.uk, and the search term 'water memory' yielded 4,140 hits. These hits can be categorised as several discrete types of contribution to the debate surrounding 'water memory'. I have identified 12 categories of material, and the graph below depicts the proportion of the first hundred hits that fall into each of those categories.

Core-set online: This category includes any materials produced online by individuals that have been active in the constitutive and contingent forums. In the case of water memory, the two 'core-set' hits in this sample of 100 are new online material from Benveniste.

Alternative science: This category includes those discussions of, or references to, water memory hosted on sites purposed to promote various unorthodox 'sciences'. These include sites focussed on astrology, the spirit of the Earth, electromagnetism and several sites dedicated to the theory of living water. These sites promote a 'scientific' interpretation of the world, without the restraints of the 'laws' that govern standard physics and chemistry. This category includes sites produced by individual enthusiasts and also sites that represent the work of various alternative science institutes, foundations and associations. In this context 'water memory' is cited either as an accepted foundational principle of the alternative approach being promoted, or is described as an example of how the main stream systematically rejects unorthodox scientific findings.

Discussion: This category includes those references to water memory that exist in online mail groups and discussion forums. In the case of water memory the majority of these forums are also dedicated to the development of alternative science. Others

are for discussion of alternative medicine, unusual or unexplained science, or general interest amateur science.

Amateur online: This category includes online publications from amateur enthusiasts. The sites do not betray any overall agenda of alternative science or medicine, and the bizarre and unexplained are not prioritised over other science news. These sites are often produced by a single individual who has posted their views on contemporary science issues online, or by groups that publish online magazines promoting amateur comment on science. The majority of treatments of water memory in this category simply describe the 1988 publication and offer amateur commentary on the perceived viability of the theory.

Homeopathy: The materials in this category appear on sites specifically dedicated to the promotion of homeopathy. These sites are produced by individuals and organisations, and are purposed to offer information and advice on the practice of homeopathy for healing. Some of these sites reference Benveniste's work as a point of interest in the subject area, while others cite the theory of water memory as demonstration, or proof that homeopathy functions on a scientific basis.

Mention in other contexts: This category includes materials that reference water memory, but without its being part of any agenda specific to the site, or its content being relevant to the nature of the site itself. For example this category includes two references to Benveniste's 1988 paper in sites offering science bibliographies. The remaining hits in this category exist as a result of Benveniste's formation of the company DigiBio. In these examples DigiBio is listed, or briefly described on several online companies registers.

Educational materials: This category includes references to water memory in online undergraduate teaching materials. In the case of water memory all the examples in this sample reference Benveniste's work as an example of rejected science.

Alternative Medicine: The references in this category appear on sites dedicated to the promotion of alternative approaches to health, healing and medicine. These are more general sites than those in the homeopathy section and include crystal healing, acupuncture, herbalism and the consumption of electromagnetically modified water. These sites either reference water memory as evidence for the use of modified waters, or as evidence for the benefits of homeopathic and herbal therapies.

Journal articles online: This category includes paper published journal articles reproduced online. The materials in this category do not appear in service to any particular agenda, or on a site promoting any particular interpretation of the material. The articles reproduced in this case include some of the follow up investigations by other authors after 1988.

Academics online: This category includes online materials produced by any academic author, who has not been part of the original core-set, and who has not published the material in a paper format elsewhere.

Weird science: This category includes those materials that appear on sites dedicated to the discussion of and accumulation of examples of strange and unexplained phenomena in science. These sites collect examples of scientific anomalies, sometimes describing and analysing them, sometimes making available the published articles online.

Water sales: This category includes sites that have been constructed to manage the online sale of 'modified water'. These sites reference water memory as scientific evidence for the healing or health benefits of the modified waters that they sell.

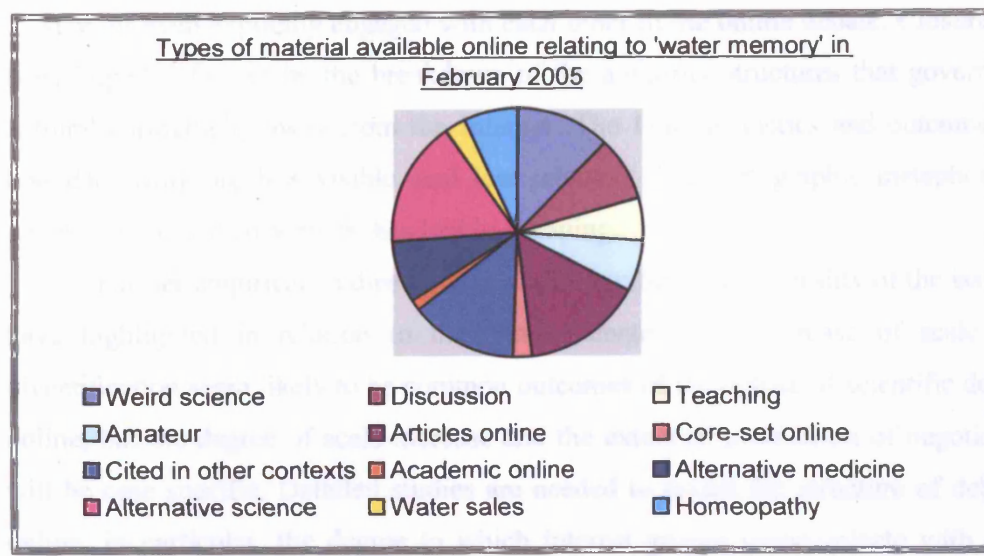


Figure 18

In the case of water memory, the online afterlife is marked by a diverse community with abundant agendas. Some groups had interests in water memory i.e. the homeopaths, and the retailers of modified waters. Others are engaged by the very notion of controversy or unorthodoxy – just like some of the online contributors to the directed mutation debate. Although constitutive forum members (i.e. Maddox at *Nature*) are happy to declare the case closed, their boundary decision remains un-agreed by the members of adjacent communities that remain vocal online.

### **5.7 Conclusion – areas for further study**

Looking at the cases of directed mutation and water memory online we see some commonalities that might indicate areas for further research. In these two cases uptake of debate in the Internet forum increased the scale of participation and extended the variety of participants. With these new participants came new interests and agendas, and the meanings of both debates diversified. This diversification and increased scale complicated negotiation, and closure became increasingly unlikely. I have shown that negotiation of these debates in the Internet forum has been ineffective, since interested parties have not necessarily agreed on appropriate

closure, or even explicitly engaged with each other in the online debate. Closure has been impeded further by the breakdown of the authority structures that govern the cultural cartography away from the Internet. The familiar tactics and outcomes of boundary work are less visible, and less relevant. The cartographic metaphor, on which boundary theory relies, has lost its meaning.

Further empirical studies are required to confirm the generality of the issues I have highlighted in relation to these two debates. The increase of scale and diversification seem likely to be common outcomes of the uptake of scientific debate online, but the degree of scale increase and the extent of protraction of negotiation will be case specific. Detailed studies are needed to reveal the structure of debates online; in particular, the degree to which interest groups communicate with each other, or even acknowledge each other will be important to understanding this diffuse community of participants. Certain key issues will need to be considered, for example:

- Do Internet debates tend to become diffuse enough to count as multiple contests?
- Are all online participants one community, or do their interests and agendas require that they be defined as related yet discrete communities?
- Are some debates more likely than others to be taken up in the Internet forum?

To make analyses of scientific debate online we must create a new methodology; I have shown that familiar tools such as core-set analysis or boundary theory cannot be applied to the Internet forum. This is a daunting task, and the result has been a tendency to ignore Internet based scientific debate and characterise it as unimportant or unscientific. I have shown that, particularly in the case of directed mutation, to ignore the Internet as a forum for negotiation means we lose a vital part of the history of this protracted debate. The most economical approach to creating a new methodology for the analysis of online scientific debate will be to attempt to salvage tools from existing methodology. I have illustrated that the loss of

the cartographic metaphor online makes some existing treatments unsuitable, however it is likely that tactics analogous to boundary work might be sought in the Internet forum. For example, the use of links online is likely to have an analogy to the constitutive and contingent forum use of references and bibliographies in the making and breaking of associations in service to authority.

Key to a new methodological approach must be an abandonment of end-directedness in our interpretations. Closure itself loses meaning on the Internet, and debates seem to acquire an immortality of the kind that Bart Simon has touched upon in his discussion of 'undead' science. The structure and dynamics of scientific debate online seem likely to require analysis that prioritises action over outcomes. In the two cases I have described here, debate has become too diffuse for closure to be a useful focus of historical or sociological analyses. In the Internet forum, perhaps even more than in other contexts for scientific debate, the notion of perpetuating forces might help us make the transition from passive accounts of controversies *en route* to closure towards richer reconstructions of the active factors influencing negotiation. Although there is a vast amount of empirical and methodological work required before we can make analyses of Internet based scientific controversy it seems that the unique combination of contingent and constitutive debate, rich communities, specialist and non specialist interaction and glimpses of the 'afterlife' of science will make the Internet very much worth the attention of historians and sociologists of science.

## Conclusion

Negotiation in the directed mutation debate has been protracted, and the controversy has not been resolved. The traditional approach to scientific controversy is end-directed and would see this episode as pending closure. Treatments of conflict in science have a teleological tendency, and suffer from problems of asymmetry akin to present-centredness. The circumstances that have perpetuated negotiation would not receive particular attention.

I have argued that protracted controversies have their own character, and that it is their perpetuation that makes them interesting. I suggest that identifying the active principles of perpetuation reveals the structure and dynamics of these complex conflicts. In this thesis I have identified six categories of perpetuating force that have been active in the directed mutation debate. These factors contributed impediments to negotiation and closure; they shaped the very nature of the debate. I argue that the language of perpetuating forces allows a more thorough analysis of long running controversies in science. I suggest that the categories I have identified, alongside possible others, might provide a tool for revealing the anatomy of protracted controversies more generally.

To enable this analysis of perpetuating forces in the directed mutation debate I have suggested some modifications to existing methodology. Sociological tools prove the most profitable for this analysis, but I have also argued that tools from the older essentialist tradition can be salvaged to bolster analyses. Gieryn's boundary work has been applied throughout to reveal the directed mutation debate as (i) an instance of the ongoing struggle for authority between Darwinians and Lamarckians and (ii) an inter-disciplinary struggle for authority between molecular biologists and evolutionary biologists. To improve the analytical power of boundary theory in this project I have pursued two of Gieryn's suggestions for extensions to his theory: I have examined the role of old maps in the negotiation of directed mutation, and I have refined the cartographic gaze to examine boundaries within science as well as those between science and the other territories.

I have provided a detailed case study of the directed mutation debate and have illustrated that this debate has two aspects – one being the immediate dissent related to the bacterial anomaly of directed mutation, the other being a

broader conflict between Lamarckian and Darwinian interpretations of evolution. I have demonstrated that the second aspect complicated negotiation and made resolution a remote possibility. I have shown that the directed mutation debate was first conducted by a core-set in the constitutive forum, but that later it was taken up by a much larger community as negotiation began on the Internet.

I have argued that the historical legacies of the directed mutation debate, and the invocation of old maps associated with those legacies, have acted as forces of perpetuation. I have demonstrated that a strongly pro-Darwinian and anti-Lamarckian context existed in the late twentieth century, and that directed mutation was necessarily negotiated in relation to that context. I have argued that late twentieth century Darwinian theory is subject to defence that is best described with Kuhn's familiar language of paradigms. That defence asserted additional requirements of proof and refutation on the advocates of directed mutation. Meanwhile, the Lamarckian implications of directed mutation forced a negative legacy of defamation on the new claims. I have shown that while twentieth century records assert the triumph of Darwinism, they also insist that Lamarckism is defeated. These identities of Lamarckism and Darwinism are recorded on old maps, that have been unfurled and deployed during the directed mutation debate. I show that the record on the old maps is not historically accurate, but rather is the sum of twentieth century boundary work to assert these identities of the two theories in service to certain groups interests. In particular, I have shown that a range of dogmas have arisen in evolutionary biology to support the old map details, and that meanwhile the actual history of Lamarckian support in the twentieth century has been obfuscated and manipulated to support the notion of Darwinian triumph. I have shown that old maps are created by boundary work and then are used for boundary.

I have demonstrated that the clash of molecular biologists and evolutionary biologists in the directed mutation debate added an additional layer to the conflict, complicating negotiation. I have described this feature of the debate as an authority struggle between these disciplinary groups, and argued that their activities can be seen as boundary work for expansion versus boundary work for protection respectively. I have argued that the different epistemologies, methodologies and practice between the disciplines made them ill-suited to conduct negotiation between them. I have shown that the groups did not agree on



methods of negotiation, or even on what might constitute closure. I have demonstrated a similar situation in the cold fusion debate.

I have argued that advocacy can be one of the most important perpetuating forces in controversy. I have shown that in the case of directed mutation John Cairn's advocacy has been a key force in prolonging the debate. I have provided a detailed biography for Cairns and a full bibliography. I have assessed his input and shown that he was a highly respected scientist, who tended towards involvement with controversy late in his career. I have characterised his advocacy as 'loud' and contrasted his style with Barry Hall's advocacy which I characterise as 'quiet'. I have demonstrated the broader applicability of these terms by illustrating an analogous case of loud and quiet advocacy in the cold fusion debate.

Finally I have shown that a key factor in the perpetuation of the directed mutation debate has been its huge change in scale and meaning as a result of uptake in what I call the 'Internet forum'. I have argued that the Internet forum should be added to the classification that identifies the constitutive and contingent forums, being recognised as a third discrete arena for the negotiation of scientific controversy. I contend that understanding the role of the Internet will be increasingly essential to understanding the structure and dynamics of late-twentieth and twenty-first century scientific debates. I have shown how the scale of Internet negotiation and the invasion of new interests that it permits, influenced the directed mutation debate, and contributed to its protraction. I suggest that neither the familiar categories of closure that science studies has relied upon, nor notions of discredit, are valid in this arena, and that we must assume a new attitude towards the ends of conflicts to understand these new circumstances. I have also demonstrated that boundary theory is not suited to the analysis of Internet based phases of debate. In the Internet forum adjacency breaks down and the cartographic metaphor is lost. I suggest that new tools will be needed for Internet analysis. I have illustrated the commonality of scale change and diversification of debates in the Internet forum through a discussion of the analogous case of the water memory controversy.

Overall, this project has aimed to contribute a novel analysis of negotiation in service to what might emerge as a specific anatomy of protracted scientific controversies. I have suggested that shifting focus from modes of

closure to forces of perpetuation might enable this richer analysis of negotiation. I have shown that existing methodology can be salvaged and put to use in that project, but that in general we must change some attitudes and assumptions regarding the nature of scientific conflict.

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## **Appendix 1: Project Design & Method**

I first heard about directed mutation during an undergraduate lecture. I was studying for a BSc in Biological Sciences at Exeter University. My final year specialist subject, evolutionary genetics, was being taught by Head of Department Professor Mark Macnair. This particular lecture focussed on the two tenets of Neo-Darwinian theory: the one being phyletic gradualism - which expresses that evolution has occurred in tiny, imperceptible, steps throughout time, rather than by sudden changes or saltations; the other being that the mutations that fuel selection arise at random, and do not have any relation to utility in respect of the demands of the environment at a given time. Professor Macnair mentioned challenges that had been made to each of these tenets. In relation to phyletic gradualism he referred to Eldredge and Gould's controversial theory of 'punctuated equilibria', and in relation to the randomness of mutation he referred to John Cairns hypothesis of environment directed mutation in bacteria. Recommended reading for that session was Cairns' 1988 'The origin of mutants' and Eldredge and Gould's 1972 'Punctuated equilibria: an alternative to phyletic gradualism'. In the context of this lecture these two cases were being raised as object lessons against challenging the tenets of Neo-Darwinian theory. The three authors were presented as mavericks, whose work had been refuted. This was in 1998.

Following the lecture I searched for Cairns 1988 paper in the journal *Nature* and was also directed by the indexing search tool to Patricia Foster and Franklin Stahl's early 1990s contributions to the debate that Cairns paper had instigated. I read a handful of these papers out of curiosity. A contradiction was immediately obvious: Cairn's directed mutation theory, which had been presented as an unsuccessful challenge to a tenet of Darwinian theory, in fact, appeared to have sparked a long running debate and presented a substantial challenge to existing theory. Having spent two years as a science student learning about the value of proof and refutation to the scientific endeavour I was surprised to find that Cairns' research was repeatable and yet not accepted.



Around this time my peers and I were designing final year projects which would form the basis for our dissertations. I had written a proposal for a study of trends in the British badger population since 1985, but, having been intrigued by Cairns' directed mutation experiments, I decided to change my project. I asked professor Macnair to act as my supervisor, and suggested that I might make a project of replicating Cairn's original Harvard experiments, with a view to judging their repeatability. The experiments would have been relatively inexpensive, using only cheap materials and taking up limited laboratory space. Professor Macnair suggested that this plan might be a precarious one, since if the experiments failed there would be little material for a write up. Instead he suggested that I take a theoretical approach, creating a literature review on the topic and discussing how directed mutation related to Neo-Darwinian theory. Therefore, over the next few months I wrote: 'The Darwinian paradigm: does modern adherence to Darwinism damage the objective aspirations of science.'

I had read the journal articles that had informed ten years of debate. Cairns' experiments had been repeated, and extended to other genetic systems and even other unicellular organisms. The molecular biological phenomenon that had been described by the Harvard team seemed to exist. However, the Lamarckian language that Cairns had used, and the assault that his research had made to Darwinian theory had created substantial dissent. While bacterial directed mutation could be demonstrated, a non-Darwinian theory for mechanism meant that it remained controversial. It was my feeling that I had uncovered an instance of science not working.

In the months after graduating from my degree programme I continued to think about the directed mutation debate, and kept up to date about developments in the debate online. Within months I had begun to design a PhD project that would allow me to study this debate further. With my training as a biological scientist the obvious choice was laboratory based research, focussing on repetition and extension of the work that had been done on directed mutation. As such my project offered to allow me not only to study the debate, but also to actually engage in it. I made contact with a number of molecular biologists and bacterial geneticists in the

London area, where I was living by this time, and met with several potential supervisors. One of the academics I met with was Dr Armand Leroi, an evolutionary biologist at Imperial College London's Berkshire-based biological sciences department at Silwood Park.. We spent an afternoon discussing possible research strategies and funding options. We discussed the non-Darwinian implications of directed mutation, and talked about the potential problems these might provoke in terms of funding. The Lamarckian associations of directed mutation were difficult to avoid. This led on to a discussion of the wider issue of the pervasive influence of Darwinism on contemporary research and the fate of Darwinian dissenters in evolutionary biology. We thought that, perhaps there might be a theoretical project that discussed these issues, and used directed mutation as a case study to examine the state of late twentieth century evolutionary biology. Before I left Silwood Park Dr Leroi made a phone call to a colleague on the South Kensington campus of Imperial College, and arranged a meeting for me to discuss a potential theoretical project on the Lamarckian versus Darwinian debate.

A few weeks later I met Dr David Edgerton in Imperial College London's Centre for the History of Science, Technology and Medicine. He explained to me the kinds of perspective that the history, philosophy and sociology of science could offer to this kind of project. The aspect of the debate that had really interested me was the core dissent between the Darwinian mainstream and the advocates of directed mutation. I realised that theoretical work of the kind that Dr Edgerton was describing would allow me to explore the conflict from that perspective. I did not have to get involved in the debate in order to offer a commentary on its dynamics. On Dr Edgerton's advice I enrolled in an MSc course in the History of Science, Technology and Medicine. From the autumn of 2000 I studied in the London Centre for Science, Technology and Medicine, which combined the resources of Imperial College London, University College London, The Wellcome Trust and the London Science Museum. During the Masters programme I gained the historiographic and methodological skills that I would need to design and carry out a project within this discipline.

During the MSc I met many historians of science, and often discussed my case study and potential project with them. It seemed that there were many ways of conducting the kind of research that interested me. Amongst these academics was Dr Joe Cain from the Department of Science and Technology Studies at University College London (UCL). Dr Cain is a historian of biology, and has a special interest in evolutionary biology and twentieth century biology. Our interests overlapped significantly, and Dr Cain's interests made him an ideal candidate to supervise my project with its focus on very recent history. Dr Cain agreed to be my PhD supervisor, and, whilst finishing the Masters programme, I set about designing my research methodology and completing a more in depth literature review of the directed mutation debate. The case study for the project came together very easily. It was intended as a non-analytical section of the thesis, which would function as a resource for analysis in subsequent chapters. It required updating periodically, since I was committed to keeping my account relevant by keeping track of the dynamics of the debate as it unfolded. The analytical challenge was to find a way to evaluate the structure and dynamics of this debate. I wanted to discuss the very nature of Darwinian dissent in the late twentieth century, and more specifically, to look at the fortunes of Lamarckism in this context. Dr Cain and I agreed that this could be best achieved by first building an account of the nature of twentieth century adherence to Darwinian evolutionary biology; that I should seek the origins of Darwinism and trace those to the modern day. Alongside this account would appear a counter history of the fortunes of Lamarckism during the same period. This strategy would serve to contextualise the different treatments of Darwinism and Lamarckism in modern biology. Problems arose in relation to this strategy. In relation to building an account of the rise of Darwinism Dr Cain and I shared the concern that this approach amounted to 'reinventing the wheel'. The literature by no means lacked an account of the synthesis period, it only lacked a version of the history told for my specific purpose. On the contrary, it appeared that a history of Lamarckism was almost entirely missing from the literature. With Darwinism in its ascendancy during the mid-late twentieth century the concurrent history of Lamarckism seemed to have been obfuscated. Rebuilding this history and rediscovering Lamarckian advocates

and their stories would be a larger project that could be attended to in one aspect of my thesis. Recovering that history would be a PhD project in itself. So, alongside a non-analytical account of the history of the directed mutation debate I was left with a derivative account of the synthetic period, and a subsequent speculative discussion of the paradigmatic nature of Darwinism in modern biology and the problems this caused for dissenters. In this format the originality criteria for the project were in jeopardy.

Dr Cain suggested that one possible solution to these methodological problems would be to include oral history in my project. The idea would be to interview evolutionary biologists and obtain their accounts of the status of Darwinism and Lamarckism in modern biology and to question them about their experiences. Perhaps, in this way, adherence to Darwinism in modern evolutionary biology might be traced as a function of teaching and training in science. The advocates of directed mutation could be asked about their experience as dissenters in evolutionary biology. I began to consider the content of such interviews and who the appropriate interviewees might be.

At the same time Dr Cain suggested that my largely historical approach might be bolstered by attention to the sociology of science, and I began to read about the methodological tools that sociology had contributed to science studies. The UCL PhD programme required me to have a secondary supervisor to support Dr Cain's mentorship. Sociologist of science, Dr Jane Gregory was suggested. I met with Dr Gregory over the coming weeks and she guided me towards some key readings in the sociology of science. In particular she recommended that I look into boundary work theory.

I read sociologist Thomas Gieryn's 'Cultural Boundaries of Science', and at once saw an alternative strategy for telling the story I was trying to construct in the thesis. Using the cartographic metaphor, as Gieryn had done to assess the relative authority of the cultural domains, I could express the history of the rise of Darwinism as a progressive struggle for extended authority. I could also contrast Darwinism and Lamarckism in terms of their relative authority and explain the difference in terms of their advocates' success or failure at having garnered cultural

authority. In the language that Gieryn offered I could frame the directed mutation debate as a challenge to the established authority structures in evolutionary biology, with the advocates and adversaries in this debate in conflict regarding the appropriate boundaries and thus authority of Darwinian evolutionary biology. The sociology of science had provided a lens through which to better view my case study and the historical legacy which had informed its context.

Dr Gregory also encouraged me to pursue oral history, particularly in the case of John Cairns, whose style of advocacy formed an important part of the case study story. In 2001 I managed to contact Professor Cairns in Oxford, where he had retired. By this time he was in his eighties. Cairns very kindly assisted me in building a thorough account of the directed mutation debate. Through emails and letters over a period of a year he directed me towards additional materials, patiently explained the details of methodological debates and guided me as I identified a key cast of characters in this debate. Cairns also offered some valuable insights into aspects of the debate that had accompanied the journal publications. He told me that he had engaged directly with a number of molecular biologists and evolutionary biologists in an exchange of letters, often dealing directly with others methodological queries or complaints without waiting for the lengthy process of journal publication to address each small point. All this provided a wonderful insight into Cairns' role as an advocate. In particular, Cairns referred me to a series of letters he had exchanged with evolutionary biologist Bruce Levin. Levin had been a harsh critic of Cairns' interpretation of the Harvard bacterial experiments. Levin's student Richard Lenski became a key critic in the journal debate, while Levin himself debated the issue directly with Cairns through private letters. Cairns had kept some record of their exchanges and was willing for me to see them with Levin's permission, but when I approached him by email, Levin suggested that the letters wouldn't be very revealing and that there was little point in my seeing them. Levin did, however, share with me some of his thoughts on methodology and the appropriate interpretation of the directed mutation phenomenon. He suggested I look at Lenski and Mittler's publications as a source of 'reason' on the subject. Around this time both Cairns and Levin encouraged me to contact Patricia Foster,

since they both considered her to be an authority on directed mutation. I subsequently exchanged some emails with Foster and she again assisted by directing me towards materials and clarifying some aspects of the molecular biological debate which by 2001-2002 had become very complicated and specialist.

Although the contacts that I had made with the 'core set' were revealing at some levels, it soon became clear that this research approach would have limited usefulness to my overall project. While both advocates and adversaries could shed light on the dynamics of this debate and reveal further details of their views than might their papers' alone, they were not able to provide material that would be helpful in the more analytical areas of the thesis. As each provided me with information they did so often with the expressed hope that I would be grinding an evaluative axe in my history of the events of the debate. At its extremes for some of the advocates this became almost an appeal for vindication, and for the adversaries an appeal for validation of their critique. Had the project just been the recreation of a rich narrative describing the events and characters in this debate, there would have been merit in pursuing oral history further. However, with the project designed to address the broader issue of Lamarckian and Darwinian histories, the notion of being taking hostage (See Chapter 1, p40) by the subjects of my study became a very real concern. So having pursued an oral history approach for just over a year I concluded that the approach had provided as much information as would be useful in this context.

Unfortunately ill health forced me to take time away from research between 2003 and 2005. I eventually returned to the thesis, working part time and at that point turned my attention to the remaining analytical chapters. Chapter 5 was intended as an analysis of the role of the internet in the directed mutation debate. I hoped to offer some general statements from this analysis regarding the role of the online community in contemporary science. Dr Joe Cain had initially encouraged me to pursue this issue, and so back in 2000 I had made a review of the debate online and I had carried out a quantitative analysis, looking at every source that referred to the debate and making a short summary of its contribution. When I returned to my research in 2005 this seemed an ideal time to make a second review, enabling a

comparison to be made between the debate online in 2000 and 2005 and thus shedding light on the dynamics of this aspect of the debate.

In 2007 I began to construct the thesis, compiling the analytical material that would stand alongside the case study chapter that I had written earlier. The case study needed some updating, but was otherwise complete. This was a chapter length narrative describing the structure and dynamics of the directed mutation debate. This had originally been intended as Chapter 1 of the thesis, with the analytical material proceeding from it. Having adopted a sociological approach, and having created a synthetic methodology from the sociology and philosophy of science, I decided to alter the thesis structure to some extent. The case study became Chapter 2, allowing Chapter 1 to be taken up with a literature review and description of my methodology, thereby providing me with the opportunity to begin by describing the merit of 'boundary theory' as a lens through which to view my case study. In the process of designing the thesis I had come to realise that boundary theory was the most broadly useful tool for analysing this debate, and that it allowed the activities of advocates and adversaries to be revealed in new lights. In Chapter 1 I described the philosophical and historical tools that could contribute to my methodology, I highlighted sociological approaches that could enhance my analysis, and worked towards the conclusion that boundary theory was overall the most practical tool for my analysis. Thus the structure of the argument throughout Chapter 1 effectively mirrored my own intellectual journey, as I had considered first historical and philosophical tools and then realised the merit of sociology and in particular boundary theory.

Making my methodology explicit at the beginning of the thesis in a detailed account allowed me to highlight the contribution that I wanted to make to boundary theory through this study. Thomas Gieryn, the architect of boundary theory, had suggested some areas for further research. In his view, attention to interdisciplinarity and to the potential stability of authority structures was required. My case study provided the perfect opportunity for exploring these themes, since it encompassed an interdisciplinary conflict, and dealt with the long term stable authority which had become attached to Darwinian theory. Thus, I also used Chapter 1 to express my

intention to extend boundary theory, bringing to the fore one of the original contributions that I hoped to make.

Chapter 2, the case study, remained a largely non-analytical narrative as I had originally intended. The account that I had first begun to construct some years earlier from key journal articles was brought up to date, and had been bolstered by additional material from some oral history investigation. Locating the case study after a chapter on methodology perhaps helped to highlight key themes for the reader: the contest for authority, interdisciplinarity and advocacy. It was my intention that this narrative serve a dual function; being first a source for the forthcoming analysis in my subsequent chapters, but also providing a stand-alone resource for other researchers who might make use of a developed case study of a debate within evolutionary biology. In that sense, I hoped that even a largely non-analytical chapter might make a contribution to the wider discourse.

In its final form the thesis was designed to have three analytical chapters, each following from the case study and developing a specific theme in relation to the that narrative. Chapter 3 dealt with the historical legacies of Lamarckism and Darwinism. As described above, this chapter had proved difficult to construct. I had discovered relatively early on that a plainly historical approach was rather redundant. With the sociological focus, that the thesis had by this time developed, the historical account of the rise of Darwinism was replaced by a sociological account of this well studied period in biology. Rather than re-stating this history for my own purposes, as I had originally set out to do, rather I used boundary theory to describe the means by which the advocates of Darwinism had sought to acquire increasing authority for Darwinism. Thus, I was able to retell this well known history from the perspective of the boundary objectives of the Darwinian advocates of this period. Also, I was able to reframe the lost history of Lamarckism as a function of the progressive rise of Darwinism and of the interests of its advocates. In this way the redundant ‘reinventing’ of the synthetic period was avoided, and this chapter was able to contribute a new perspective on this history as well as putting boundary theory to work against a well known historical case of authority struggle.



In this chapter I also tackled one of the extensions to boundary work that I had outlined in Chapter 1: I demonstrated that the authority of Darwinism has not been local and episodic, but rather has been stable over a long period and throughout a number of authority contests. I used the language of ‘old maps’ to describe the unquestioned, tacit assumptions that lead to the stability of some authority allocations. Thus I was able to provide a rich sense of the context for non-Darwinian work in the late twentieth century, as I had originally hoped to do in this chapter.

Having used history and sociology in Chapter 3 to demonstrate the difficulties with carrying out non-Darwinian work in the late twentieth century, I wanted to explore the possible motivations of researchers taking on this daunting task. In particular I wanted to ask: why be a Lamarckian in the late twentieth century? In Chapter 4, using the directed mutation case study as a starting point, I explored advocacy as a means by which to understand the interests of non-Darwinian researchers. My analysis was based on a close study of the advocacy undertaken by John Cairns during the directed mutation debate. In order to achieve this I provided a complete biography and bibliography for Cairns, seeking to contextualise his work on directed mutation within his long and prestigious career. It was in this task that the oral history I had undertaken proved perhaps most useful. My communication with Cairns allowed me to piece together a very complete account of his multinational career, a task that would not have been possible from secondary sources alone. I argued that there are different styles of advocacy, and used a supporting case study to show the greater generality of the styles of advocacy that I had identified in the case of directed mutation. I argued that some styles of advocacy were more suited to non-Darwinian work than others.

In Chapter 4 I also addressed the issue of interdisciplinarity. The narrative case study had shown that an interdisciplinary contest for authority was at the heart of the directed mutation debate. I extended the language of boundary theory to describe this contest between specialist groups within biology. As suggested in Chapter 1, I extended the value of boundary theory to analyses of contests within science as well as between science and other areas of culture. To achieve this I offered a characterisation of molecular biology and evolutionary biology, and

considered the stakes and interests related to conflict within each of these disciplines. I was able to show that boundary theory has the capacity to explore the circumstances of disputes within science.

Using the data that I had collected in 2000 and 2005 I constructed Chapter 5 as a largely empirical study of the change of scale and content conferred on the directed mutation debate by its uptake in the Internet forum. I argued that scale change and diversification are likely to be outcomes of uptake of debates online, due to the nature of participation on the Internet. I included a study of water memory and homeopathy to demonstrate the generality of the themes that I had discussed. In this chapter, I also reviewed the attempts that have been made to visualize the Internet, and argued that the nature of this new environment for debate makes much of our existing methodology for conflict studies redundant, and its language obsolete. This had become apparent to me through the process of attempting to apply the terms of boundary theory to this new space. The scope of this thesis did not allow me to offer resolution of these methodological problems, but I did highlight some ways in which the Internet might be studied, and some appropriate questions that might be asked about scientific debate online.

Making this range of analyses in relation to directed mutation created an eclectic thesis. I had highlighted points of interest from my case study debate and explored a range of these in detail. The unifying feature of my analyses had occurred to me when designing the content of Chapters 3 and 4. What had originally interested me in this debate had been its protraction. It seemed unsolvable, since, while science was borne out through repetition, agreement concerning legitimacy could not be reached. It occurred to me that the points of interest that I had identified in the debate were in fact the very agents of the protraction. To create a unifying theme for the project I was thus able to frame the project as a search for the features that had perpetuated the directed mutation debate, and impeded its resolution. Thus, the chapters subsequent to the case study each reveal particular aspects of the debate that I term 'perpetuating forces'. Overall, this has given me the opportunity to argue that long running debates in science have a particular character, and that that character is conferred by the active forces which prolong the debate. Throughout the

thesis I hope to have made a contribution to the ongoing study of scientific conflict by showing that the perpetuating forces I have identified in this project are perhaps part of a common anatomy of long running debates.

In conclusion, what appears in the final thesis had been achieved by a number of methods. The case study had been constructed from literature review, and further details provided by oral history. An account of the history of Darwinism and Lamarckism had been reconstructed from secondary literature viewed through the lens of boundary work theory. An account of the role of advocacy in the debate had been constructed partly from a discourse analysis of primary journal materials, and had been bolstered by Cairns' oral history account. Cairns' biography, which supplemented the chapter dealing with advocacy, had been constructed from literature review and Cairns' own account of his contributions. Finally, the chapter dealing with the role of the Internet had been constructed from an empirical survey carried out between 2000 and 2005. I stress in Chapter 1 of this thesis that a combined methodology has been used to allow the analysis undertaken in this thesis. I suggest here that combined methods have also been key to the approach taken in this research.

## **Appendix 2: The Directed Mutation Debate: Cast of Characters**

**David Baltimore** - Molecular biologist. Discovered the enzyme reverse transcriptase simultaneously with Howard Temin in 1970. This enzyme allows the transfer of information between RNA and DNA, undermining the dogma of unidirectional information transfer from DNA via RNA to protein that Francis Crick had asserted. Crick's dogma barred the route for environment communication with DNA and thus for environment control of mutation and ultimately evolution. Reverse transcriptase opened part of the route of communication, making environment directed mutation potentially possible. Thus, reverse transcriptase was often cited by advocates of directed mutation as a possible factor in the mechanism of the process they described.

**John Cairns** – Molecular biologist. Esteemed virologist, public health scientist and cancer researcher. Cairns was director of the Cold Spring Harbor Laboratory of Quantitative Biology during its 1960s heyday. See Chapter 4 for a full biography and Appendix 3 for a full bibliography. Lead advocate in the directed mutation debate. He proposed and defended the 'strong' versions of directed mutation theory, advocating non-Darwinian, and possibly even Lamarckian, interpretations of the bacterial phenomenon that his team first identified in 1988. He took on many duties as an advocate; answering critics in journals and through private correspondence, recruiting supporters, proposing mechanistic theories and repeating and confirming others' experiments. As advanced age distanced him from active research his mantle was taken on by Patricia Foster, his former graduate student.

**Francis Crick** – Molecular biologist. Co-discoverer of the structure of the DNA molecule. In 1970 he asserted the unidirectional transfer of information from DNA via RNA to protein, calling this 'the central dogma of molecular genetics' [Crick, F. (1970)]. That dogma barred the route for environment communication with / and modification of the DNA, thus precluding environment directed mutation of the kind

that Cairns proposed in the late 1980s. Crick's dogma was undermined within a year when Howard Temin and David Baltimore simultaneously discovered reverse transcriptase, an enzyme which allowed information transfer from RNA to DNA.

**Max Delbruck** – Biophysicist / Microbiologist. Collaborated with Max Delbruck at Cold Spring Harbor during the 1940s to create the 'fluctuation test' [Luria, S. & Delbruck, M. (1943)]. This test was designed to demonstrate that bacterial mutation and evolution was random with respect to the specific demands of the environment. They wanted to prove that pressure from the environment to achieve a certain mutation, and thus adaptation, did not increase the likelihood that the mutation would occur. They wanted to demonstrate the Darwinian assertion that mutation is random and that adaptation is based upon chance mutational events coupled to selection; that evolution is not directed by the environment. Their test became the key proof for this tenet of Darwinism. In the 1980s the advocates of directed mutation had to overcome the refutation of their findings by the precedent of the Luria and Delbruck assay. Advocates of directed mutation identified problems in Luria and Delbruck's design and argued that the fluctuation test did not refute the kind of environment directed mutation that Cairn's team had identified. Invocation of the fluctuation test by critics of directed mutation, versus defence of directed mutation theory against the precedent of the fluctuation test, characterised the first years of the journal based debate that I describe in Chapter 2.

**Patricia Foster** – Molecular biologist. Graduate student in Cairn's Harvard laboratory during the 1980s. Co-authored with Cairns on several occasions, supporting his strong versions of directed mutation theory. Progressively took over from Cairns as lead advocate of directed mutation during the 1990s. Working throughout the mid – late 1990s to uncover details of mechanism for the bacterial system that Cairn's Harvard team had identified in 1988.

**Barry Hall** – Molecular biologist. Worked on a process of adaptive mutation in bacteria that was essentially the same as the activity that Cairn's team would term

‘directed mutation’ in 1988. Hall conducted his research on this phenomenon without inciting controversy. He did not emphasise non-Darwinian implications of the bacterial system, and made no mention of the Lamarckian echoes that Cairn’s team would later highlight. Hall continued to work on directed mutation after Cairn’s team made the subject controversial, but did so with a moderate approach, seeking to interpret the system within Neo-Darwinism. Hall’s advocacy of directed mutation complemented Cairn’s. He offered a weak model alongside Cairn’s strong model, and made directed mutation more palatable to Neo-Darwinians. Hall became the lead advocate of the hypermutation model of directed mutation. This model allowed that the mutations occurring be determined to a degree by the environment, but limited this kind of genetic behaviour to bacteria and some other unicells. Hall’s hypermutation model has been favoured by advocates of directed mutation since the mid 1990s, largely supplanting Cairn’s more inflammatory strong model. Hall’s hypermutation theory for directed mutation has been largely accepted as a feature of bacterial genetics.

**Richard Lenski** – Evolutionary biologist. Trained in Bruce Levin’s laboratory. Lenski co-authored several papers on directed mutation with his former PhD student John Mittler. From 1990 these two evolutionary biologists were the principal critics of Cairn’s research and of the non-Darwinian interpretation of directed mutation. They persistently recalled the precedent of the Luria and Delbruck assay as evidence against directed mutation. They encouraged a reinterpretation of the data within the framework of Neo-Darwinism and argued strongly against any invocation of Lamarckism.

**Bruce Levin** – Evolutionary biologist. Trained Richard Lenski, who in turn trained John Mittler. These three evolutionary biologists were the key critics of directed mutation during the 1980s and 1990s, arguing against any Lamarckian interpretation of the Harvard findings. While Lenski and Mittler co-authored many papers on the subject, Levin instead engaged directly with John Cairns in a prolonged private exchange of letters concerning directed mutation.

**Salvador Luria** – Microbiologist. Collaborated with Max Delbruck at Cold Spring Harbor during the 1940s to create the ‘fluctuation test’ [Luria, S. & Delbruck, M. (1943)]. This test was designed to demonstrate that bacterial mutation and evolution was random with respect to the specific demands of the environment. They wanted to prove that pressure from the environment to achieve a certain mutation, and thus adaptation, did not increase the likelihood that the mutation would occur. They wanted to demonstrate the Darwinian assertion that mutation is random and that adaptation is based upon chance mutational events coupled to selection; that evolution is not directed by the environment. Their test became the key proof for this tenet of Darwinism. In the 1980s the advocates of directed mutation had to overcome the refutation of their findings by the precedent of the Luria and Delbruck assay. Advocates of directed mutation identified problems in Luria and Delbruck’s design and argued that the fluctuation test did not refute the kind of environment directed mutation that Cairn’s team had identified. Invocation of the fluctuation test by critics of directed mutation, versus defence of directed mutation theory against the precedent of the fluctuation test, characterised the first years of the journal based debate that I describe in Chapter 2.

**John Mittler** – Evolutionary biologist. A student of both Richard Lenski (PhD) and Bruce Levin (Postdoctoral research). Mittler co-authored several papers on directed mutation with Lenski. From 1990 these two evolutionary biologists were the principal critics of Cairn’s research and of the non-Darwinian interpretation of directed mutation. They persistently recalled the precedent of the Luria and Delbruck assay as evidence against directed mutation. They encouraged a reinterpretation of the data within the framework of Neo-Darwinism and argued strongly against any invocation of Lamarckism.

**Susan Rosenberg** – Molecular biologist. Joined the directed mutation debate in the mid-1990s working to provide much needed mechanistic data. Her prolific experiments generated a vast amount of new empirical data, giving the debate a

much needed boost in this period. Alongside Patricia Foster, Rosenberg became a lead advocate of directed mutation theory after 1995.

**Franklin Stahl** – Molecular biologist. During the 1950s Stahl discovered the mode of replication of DNA, alongside geneticist Matthew Meselson. During the directed mutation debate Stahl wrote three important review articles on directed mutation. [Stahl, F. (1988); Stahl, F. (1990); Stahl, F. (1992)] In these he encouraged that scientists be open minded in their appraisal of directed mutation, so as not to overlook exciting innovations on account of adherence to dogma. He offered his own theoretical mechanism for directed mutation, bolstering the credibility of Cairns findings.

**Edward Steele** – Molecular biologist / Immunologist. During the 1970s and 1980s Steele developed the controversial theory of acquired immunity. This suggested that immunity acquired during an organism's lifetime could alter its DNA and thus be passed on to offspring generations. This presupposed a route of communication between the environment and the DNA. Acquired immunity defies the rules of Darwinism, particularly Crick's central dogma, which has barred the route for communication between the environment and DNA. More significantly, in Steele's presentation acquired immunity represents a vindication of Lamarckian principles, and illustrates that an entirely different interpretation of evolution may be required. Steele invoked Temin & Baltimore's reverse transcriptase enzyme as the agent of the environment to DNA communication. His theory has now been widely accepted, although his Lamarckian styling for the system has been rejected. Steele began a priority dispute with Cairns in the late 1980s, after Cairns invoked reverse transcriptase in his proposed mechanism for directed mutation. This dispute was amicably resolved shortly afterwards [Steele, E. & Cairns, J. (1989)]

**Howard Temin** – Molecular biologist. Discovered the enzyme reverse transcriptase simultaneously with David Baltimore in 1970. This enzyme allows the transfer of information between RNA and DNA, undermining the dogma of unidirectional



information transfer from DNA via RNA to protein that Francis Crick had asserted. Crick's dogma barred the route for environment communication with DNA and thus for environment control of mutation and thus evolution. Reverse transcriptase opened part of the route of communication, making possible environment directed mutation. Thus, reverse transcriptase was often cited by advocates of directed mutation as a possible factor in the mechanism of the process they described.

### **Appendix 3: Bibliography for Hugh John Forster Cairns (1948 -2004)**

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